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Placental Adhesion as a Cause of Hour-Glass Contractions and Protracted Labors. By JOSEPH MARTIN, M.D., of New York.

No part or appendage of the uterus demands more serious consideration, during gestation and parturition, than the placenta. Its position in the organ, its premature detachment, its adhesion and other morbid conditions, together with its final removal, are all matters of grave import, and have long engaged the attention of the leading practitioners of midwifery. Of the abnormal conditions of the placenta, and their consequences during labor, none seem to be so little regarded, and perhaps, I may add, so little understood, as those of its adhesion to the walls of the uterus. A few remarks, therefore, on that peculiarity, illustrating opinions which are the result of much experience and patient observation, may not be uninteresting to the profession.

"Parturition is a physiological process, in which wonderful and ample provision is made for favorable results, in nearly all cases; and a profound acquaintance with that physiology is the only guarantee of success, when deviations from natural labors do occur."

What, then, is the physiological process of labor, so far as concerns the relations of the uterus and placenta, in connection with the child? In a few words: When the ovum leaves the fallopian tube, it becomes attached to some part of the uterus. And, as that organ consists

almost entirely of muscular fibres, nerves, arteries, veins, and cellular tissue, the principal connection is between its muscles and the surface of the placenta. During the fœtal development, the placenta expands with the uterus, and at the full period of gestation is still attached to the muscles of that organ.

During parturient pains, the muscles of the womb, commencing always with the circular fibres of the fundus, contract in every part of the superior portion of the organ, and with the same regularity and effective force at the place of placental attachment as elsewhere; whether that attachment be to the posterior wall, the sides, the anterior wall, or the fundus. By this wise provision of nature, the vessels of the womb, at their points of attachment to the placenta, are gradually closed, without being broken, by the contractile power of the surrounding muscular texture. This part of the process has been graphically described by Sir Charles Bell, who says: "The placenta, when it is attached in a regular circle to the fundus, cannot be partially or bodily separated, until the uterus be permitted to have a great degree of contraction by the delivery of the child; the circular muscles of the fundus being agents in a double capacity, that is, both in expelling the child and constringing the vessels."

As the labor progresses, the constringing fibres diminish the size of the blood-vessels and sinuses, and the *bruit de soufflet* is altered. The rasping or sawing sound diminishes in harshness and loudness; and the drone, mentioned by Dr. Kennedy, is less distinct. This may be easily explained if the opinion of Dr. Corrigan, of Dublin, be correct, that "the *soufflet* is caused by currents of blood passing from narrow orifices into wider tubes, as in some forms of aneurism."

In all physiological labors, too, the umbilical cord, being forced down with the other contents of the womb, preserves its normal form, so that the fœtal circulation and nutrition continue at the close of parturition, as perfect as at the beginning.

In natural labors there is also a uniformity of shape and of position of the uterus and its contents, at each stage in the progress of the child's head through the pelvis. In the earlier part of the first stage, when the occiput, in the first position, is about to sink beneath the brim of the pelvis, the whole organ presents to the eye, and to the careful manipulator, a uniform ovoid tumor, no part more depressed or more prominent than any other. The same is observed when the head passes into the pelvis, except that the uterine tumor is brought more forward, and is more globular. When the occiput is under the arch of the pubis, the same regularity of form is preserved. In

no one of these different stages will the form of the tumor, during pain, indicate the place of placental attachment.

We will now notice the changes produced by a morbid adhesion of the placenta during parturition. The pathology of the diseases of the placenta is still an open question. Brachet asserted, twenty-five years ago, that all the changes observed in that organ were the results of inflammation. Cuveilhier, who followed him, referred these diseases to hypertrophy, atrophy, hydatids, apoplexy, ossification, and especially to inflammation; while Dr. Robert Lee denies that inflammation has any connection with them, and disputes the existence of fatty degenerations, so much insisted upon by Dr. Barnes and other distinguished accoucheurs as a frequent cause of abortions. Dr. Barnes is of the opinion that, "in true placental adhesion, there is organic change of structure, or abnormal deposit in the decidual portion of that organ; while, in more frequent cases, the adhesion is merely the result of ineffectual contractions of the uterus." Dr. Simpson, on the contrary, has, in my opinion, satisfactorily shown that all morbid, firm connections of the placenta with the uterus, whether partial or complete, are the results of inflammatory action. He says: "The intimacy or strength of morbid adhesion between the placenta and uterus is liable to vary greatly, according to the degree of acuteness of the inflammatory action which has produced the effusion of coagulable lymph, and the length of time it has been deposited. Occasionally, the lymph assumes a fibrous, tendinous, or calcareous appearance." It may be added, that Morgiani, and other writers on morbid anatomy, state that portions of the placenta are sometimes, but *very rarely indeed*, so firmly attached that they cannot be separated, either by the hand during life, or by maceration or by the knife after death.

But, whatever may be the pathological condition of the adherent placenta, it is evident that the connection must be between its surface and the uterine muscles. For there can be no morbid condition of the circulation on the part of the uterus—no local error of secretion in the uterine membrane—no abnormal accumulations about the fœtal tufts. Because the firmest adhesion is found in the healthiest females, carrying to the full period of gestation the largest and best-developed infants. It is also well known that, in such cases, the muscles in connection with the adhering placenta readily contract when the separation is made. It follows that, during the entire process of parturition, the muscles of a portion of the organ are firmly held by a completely adherent after-birth, in the same expanded state in which they were when the labor commenced.

Let us consider, for a moment, what must be the effect of such a morbid condition upon the contracting organ, and we shall find that, as a natural compensation, the remaining three-fourths of the uterine muscles will be excited to undue action in their efforts to expel the child. The nervi-motor power of all the fibrous tissues of the organ will be concentrated in a part, while an inert mass impedes their action at every effort. Hence, as I shall presently show, the irregular and inefficient pains, the so-called "spasms;" hence the lingering, protracted labor and consequent exhaustion of the mother; hence the partial contractions of a portion of the circular fibres, found after labor when the adhesion is below, and the hour-glass contraction found when it is near or at the fundus.

I have not yet been able to find in European books or periodicals any satisfactory explanation of partial or hour-glass contractions of the uterus; neither have I met with a single foreign writer who has hinted at the possibility that these abnormal conditions of the organ have any such connection with the adhesion of the placenta as cause and effect; although they are often described as being found, in some way, associated together; while all admit that such contractions are of great importance, because they involve more or less danger from hæmorrhage.

It will therefore be sufficient to say that hour-glass and other irregular contractions of the womb are considered by nearly all systematic writers on midwifery as "irregular or spasmodic uterine contractions, occurring after labor," as will be shown by the following extract from Dr. W. T. Smith, one of the most recent authors. He states that "in these cases, *after the expulsion* of the child, and even of the placenta, there is a double departure from a physiological condition of the uterus: there are both spasm and inertia. The contracted part of the uterus leaves the other portions relaxed, so as to admit of the flow of blood from the mouths of the vessels on the inner surface of the organ. In this way, one lateral half of the uterus will be contracted, while the other half will be relaxed. Such irregular contractions depend upon excessive nervi-motor action, which may be induced by any of the causes of acute, irregular, or tardy labors." And he adds: "Perfect hour-glass contraction is of rare occurrence, but it most generally occurs *after* rapid parturition." I have met with one English writer who is not satisfied with the prevailing opinion as to the cause and effects of these abnormal uterine contractions. Dr. Richard Hodges, of London, who published, in 1851, upward of 1,000 cases of labor, considered with especial reference to the hour-glass

contraction, professes himself "unable to give a satisfactory reason for its occurrence."

An opinion, however, generally prevails among American obstetric practitioners that hour-glass contractions cannot take place in the absence of placental adhesion. But I have seen no attempt to explain the manner in which they are produced, except in Dr. Charles D. Meigs' Treatise on Midwifery. "I cannot conceive," he says, "of an hour-glass contraction independently of a preternatural adhesion of the after-birth to the womb. *I suppose*," he adds, "that when the after-birth is so firmly attached that the contractions of the womb cannot slide it off, the substance of the placenta acts as a soft splint, counter-extending the utero-placental superficies. The rest of the womb, having nothing to antagonize it, contracts as usual, leaving the placenta shut up in an upper cell."

I have long entertained the opinion, based upon analogy and observation, that abnormal uterine contractions, found after delivery, are produced, in some way, by an energetic action of those muscles that aid in expelling the child. I considered the generally-received opinions, to say the least, exceedingly indefinite and unsatisfactory; and believed that obstetric writers employed the terms "spasm and inertia" in reference to the uterus, without reflecting that neither spasm nor inertia can exist in that organ in the absence of some abnormal condition of its muscular fibres.

Impressed with the practical importance of these views, I have endeavored to confirm their correctness by facts that could not be controverted; but, until recently, in vain.

I will *now*, however, adduce evidence to *prove* that all contractions of the uterus, above the os, which are found tonic and persistent *after the expulsion of the child*, whether the placenta may have been thrown off or not, *existed before delivery*, and are the results of an undue action of a portion of the muscular fibres, in their efforts to expel the child; that the irregular action is induced by the greater or less inertness of a part of the uterine muscles, consequent upon their partial or complete adhesion to the placenta; that, in all cases of placental adhesion, irregular uterine contractions must take place during parturition, whether they be found after delivery or not; that labors attended by such abnormal conditions of the uterus and placenta are more or less protracted; and that those morbid attachments are more frequently the causes of dangerous hæmorrhage, before and after delivery, than is generally supposed.

As a proof of the correctness of these views, I will relate, as

briefly as is consistent with a proper understanding of the subject, a case of hour-glass contraction of the uterus, and two of partial contraction of that organ, which have recently come under my notice.

On the 3d of February, 1858, at 3 P. M., I was called to see Mrs. G., residing in Brooklyn. She was at the full period of gestation, with her sixth child. Age, 27 years; general health good; pelvis well formed; temperament nervo-sanguineous. She had some pains; but, as there was no presental tumor, and the os uteri not open, I left her for eight hours. On my return, at 12, there was still no presentation. The os uteri received the finger, but no part of the child could be felt, although what she called labor-pains continued during my absence. On examination of the child through the abdominal and uterine parietes, I found the face turned downward, over the pelvis, with the os frontis resting upon the brim, above the left acetabulum, and the fundus uteri far over to the right side of the abdomen; in which position, she told me, it had been for a number of weeks. This was caused, no doubt, by the weight of the placenta, which I found to be at the fundus. By long-continued efforts during pains, with one hand under the fundus and the other over the occiput, the head was made to partially enter the brim of the pelvis.

At 6 A. M. on the 4th, the os uteri was relaxed, and open to the size of a quarter of a dollar. The fundus continued to incline, more than usual, to the right side. The occiput could be felt above the brim, and the left shoulder above the pubis; the fœtal pulsation was distinct; the placental *soufflet* was unchanged, and was spread over a large surface of the fundus.

At 8 P. M., twenty hours after the commencement of labor, the pains became irregular, both as to force and interval, and very inefficient. There was, occasionally, a forcing pain; at other times, as the patient lay upon her back, the uterine fibres would slowly raise the loaded fundus from its position at the side, and draw it over the pelvis for an instant, when it would fall back as if it were an inert mass. Again, there were violent muscular efforts, apparently spasmodic, which had little or no effect upon the fundus. Chloroform was used, but, after giving it a fair trial, it was discontinued.

At 11 P. M., as she was sensible of the inefficiency of her pains, although she described them as exceedingly "severe and cutting," she became alarmed. Some delirium, with other unequivocal signs of convulsions, being observed, I bled her from the arm, and used the cold douche freely. By these means she was composed, fell into a

sound sleep, and continued to rest, with only an occasional slight pain, for ten hours.

At 12 M., the 5th, thirty-six hours after the commencement of labor, the pains were regular, and apparently forcing; but they had very little effect upon the child. The uterine tumor looked longer and larger than it did the day before, and seemed as if it were imbedded in the intestines. The fundus was still hard and round, without any change in size. The *bruit de soufflet* was as loud and as distinct as it was at the beginning of labor. The middle zones were more irregular, and softer than usual. When examined with the hand, fluctuation was sometimes perceptible; at other times, tympanitic sounds could be heard; and at others, auscultation detected a gurgling sound. These peculiarities were observed on the anterior as well as on the lateral portions of the organ, during pain, as well as in its absence.

During the next eighteen hours, no important change could be perceived; and she seemed to be doing very well until 6 A. M., on the 6th, when she became excited and discouraged. Cold water was promptly and freely used, and appeals were made to her courage and resolution, and she was reassured. As the pains, however, were irregular and inefficient, I proposed to operate; and encouraged her by the promise of a safe and speedy delivery, together with the possibility of saving her child, which she knew was still alive. I then sent for a physician in the neighborhood, (Dr. Davis,) who came, and assisted me with tact and judgment.

After she was placed under the influence of chloroform, I proceeded to operate; but found that the blade of the forceps could not be passed between the parietal bone and the pubis without doing injury to the soft parts. The right blade of the forceps was then passed over the occiput, and the head was brought down, with the assistance of pains, until the vertex was near the arch of the pubis. On introducing the fingers, with the intention of delivering at once, I found the child still alive. As the soft parts offered no impediment, and as there is, during labor, no unequivocal diagnostic sign by which we can decide that the funis is around the child's neck, I waited a few minutes, for the purpose of observing the effect of one or two pains, and giving the infant one more chance for life. Finding that the head did not advance, I applied the forceps, the right-hand blade passing under the ramus of the right ischium, and over the right ear. When the head was delivered, the umbilical cord, now stretched to a

smooth, flat, tape-like string, without pulsation, was entwined about the child's neck, effectually strangling it.

After dividing the cord, I held it in the left hand, while the right was placed upon what I thought was the fundus uteri. But, finding no contraction, and seeing bright, florid blood flowing in a stream from the vagina, I at once introduced the hand into the uterus. The cord guided me to a round, central opening, just large enough to receive the points of three fingers. The hand having been cautiously pressed through the opening, was inclosed within a flattened globe or spheroid, by a firm grip of the contracted muscles upon the wrist. To the upper concave surface of this globe a circular placenta was firmly adhering. The thinnest part of it, at its periphery, had been turned inward, and formed the inner surface of the globe at its greater diameter. That peripheral portion had been separated from the uterine blood-vessels, from which the blood flowed upon my fingers. With the left hand upon the fundus, which was now pressing against the right ribs, the adhering mass was carefully peeled off. The right hand, grasping the placenta, was then gently rotated upon the central surface, while friction and pressure were made externally by the other hand over the same part. Contraction immediately commenced, and the flooding ceased. As the contracting circular muscles forced the hand, with the placenta, towards the hour-glass contraction, it relaxed, and, receding from the wrist and hand, it disappeared, as the upper part of the organ assumed its normal concave form. The entire organ now contracted, and very soon expelled the hand and afterbirth into the vagina, from which they were at once removed—fifty-seven hours after the head had been directed into the superior strait. She experienced no uterine or peritonitic derangements, and had a good, though slow, recovery.

In this case, the circular fibres of the fundus uteri could not have aided, in the slightest degree, in forcing the head of the child into the upper strait, for they did not follow the breech; nor did they contract, for the blood-vessels which they surrounded were not constringed, as the *bruit de soufflet* remained unchanged thoughout the whole process. But the history of the case, and all the attending circumstances, *go to prove* that the circular uterine fibres, which were free to act near the periphery of the placenta, contracted slowly around the breech of the child at every pain, until they formed a new fundus, or a substitute for the natural one, which, acting in connection with the muscles of the lower part of the organ, made efforts to expel the child; and that a deep circular sulcus or depression, occupied by

intestines, was thus formed in the walls of the uterus, between the nates of the child and the globular apartment containing the placenta.

That this hour-glass contraction could have taken place during, or after the delivery of the child, is *simply impossible*. *First*. Because the edges or peripheral parts of the placenta were separated from the uterine blood-vessels, which must have been effected during labor by the contractile power of the free muscular fibre, near that body; for blood in large quantities came with, and followed the child. *Second*. Because the extraction of the child occupied but a few minutes, and the hand was passed up to a firm band of contracted muscular fibres in less than one minute after. *Third*. Because the softness and fluctuation, the gurgling and tympanitic sounds, which could have been caused only by the presence of the intestines, and which were found on the anterior as well as on the lateral portions of the uterine tumor, were repeatedly observed for hours before delivery. The abnormal condition of the uterus must, therefore, have been slowly produced during labor; so that it may safely be asserted that, in this case, the placental adhesion was the cause of the protracted labor, and of the hour-glass contraction.

For the history of a similar case of placental adhesion and hour-glass contraction, in a subsequent labor of the same lady, after an interval of fifteen months, see the last July number of this journal.

But it is not in all cases that lingering labor attends placental adhesion and complete hour-glass contraction. For in a pluripara case, with a large pelvis, strong muscular fibres, and comparatively small child, there would be a quick labor, although the muscles of the fundus might not act. Because the contraction of the circular fibres below the adhesion, which I contend are brought into action as a substitute, would be sufficient to expel the child in a few hours. Mrs. G's case would have ended favorably to the child, and without instrumental aid, in the absence of all complication with the cord, but could not have been a rapid labor. Whether such cases are lingering or not, is, however, of very little practical importance, as complete hour-glass contraction is of rare occurrence. Dr. Richard Hodges, in reporting his 1,000 cases of midwifery, records but two marked by that peculiarity. Professor Barker, of this city, has kindly permitted me to state, that in upward of 2,000 cases of midwifery, of which he has had the management or control during the last seven years, he has met with but three true hour-glass contractions. In the course of a practice of twenty-five years, with especial attention to midwifery, the writer has met with only one case of the kind. Doctors

Hardy and McClintock state, that in treating 6,634 women, delivered in the Dublin Lying-in Hospital, from January 1st, 1842, to January 1st, 1845, they met with only "one instance in which hour-glass contraction of the uterus was the cause of the placenta being detained."

Still, it has been proved by the history of Mrs. G's case, that when there is a firm adhesion of the placenta to the circular muscles of the fundus uteri, rendering them perfectly inert, the other muscular fibres of the organ are brought into requisition at the period demanded by nature; and that they then supply the place of the inert fibres in a most wonderful manner. It may, therefore, be safely inferred that, when a placenta is thus planted upon any other part of the organ, a similar effort of nature will be made to accomplish the same end. It is in such positions that this abnormal condition of the after-birth is a fruitful source of tediousness and danger, as I shall now proceed to show.

The result of the observations of Ritgen, Carmichael, Naiglae, and Dougherty is, that the average proportion of placental attachments to the os uteri is 2 per cent.; to the fundus, 3 per cent.; to the anterior wall, 5 per cent.; and to the posterior and lateral walls, 90 per cent. Ritgen found, on examination after delivery, in 100 cases of midwifery, that the membranes were broken at *the edge* of the placenta in 22 cases; and at one inch from the placental edge in 8 cases. In 50 cases, the greatest space between the os uteri and the edges of the after-birth was 4 inches. In 90 cases, that space was 6 inches; and in 100 cases, the greatest space between the os uteri and the placental edge was 8 inches.

These statistics explain the infrequency of perfect hour-glass contractions of the uterus; for it is evident that they cannot occur above the cervix, unless the placenta is adherent at or near the fundus. They also show that in every 100 cases of midwifery, there are 90 in which the placenta will be attached to the posterior or lateral walls of the womb, below the fundus, and within six inches of the os uteri.

Adhesion of the placenta is either perfect, as in Mrs. G's case, or it is partial. When it is partial, a part of its lobules may be firmly adherent, while the others are physiologically attached; or the whole mass may adhere imperfectly.

I contend that when there is, during labor, a perfectly adherent placenta on the posterior, or on either of the lateral walls of the uterus, the nervi-motor power of the other three-fourths of the organ is taxed to perform the office of the whole. Because, in such a case, pains are felt, the uterine tumor is lessened in size, the child advances,

and in most instances, if there be no interference, it is expelled from the womb. But after delivery no contraction of the uterine muscles can be found at the place of placental adhesion; they are found in the same expanded state before the adhering mass is artificially removed that they were at the beginning of parturition. For the *bruit de soufflet* is unchanged, and the mouths of the uterine arteries in connection with the placenta are not constringed by the encircling fibres; while the unrestrained muscular fibres in almost every other part of the organ are found in a state of abnormal contraction.

If such be the pathological condition of a completely adherent placenta during parturition, and such the cause of abnormal uterine contractions, the consequences will be an irregular form of the uterine tumor during pains, and after delivery; the absence of all change in the placental *soufflet;* irregularity in the action of the fundal muscular fibres; a tonic semicircular contraction, in most cases, of the muscles opposite to and near the adhesion; often, a circular contraction of the fibres below the adherent mass; and a lingering labor, attended by more or less hæmorrhage. In some cases, however, the uterine muscles that are excited to irregular contraction during labor, relax soon after delivery, and no tonic contraction is found when the hand is introduced; while adhesion continues, and some flooding ensues, consequent upon a partial separation.

The same results attend the two forms of partial adhesion, except that the labor is generally less lingering, and there is greater danger from hæmorrhage. When the entire surface of the after-birth adheres imperfectly to the posterior, or either of the lateral walls of the organ, it is not infrequently thrown off in a mass at the close of labor, by the action of the free muscular fibres; then is found the *half* hour-glass contraction, described by Dr. W. T. Smith, and dangerous flooding occurs. When such an adherent placenta is at the fundus, it may be separated *in the same manner*, and retained after delivery, by a *true* hour-glass contraction. This was the case in one of the labors with hour-glass uterus reported by Dr. Hodges. If one or more lobules of the placenta morbidly cling to the surface of the uterus, while the others are slightly adherent, or normally attached, the latter will often be thrown off by the irregular contractions; and blood before, or at the moment of delivery, will freely flow from the vessels left open by the separation.

That there are many lingering labors, even in pluripara cases, when the condition of the patient and all the attendant circumstances justify the expectation of a safe and speedy delivery, is proved by the records

of medicine, and the general experience of obstetric practitioners. It is also equally true, that such cases are generally attended by hæmorrhage, before or after the extraction of the after-birth, which often requires to be artificially removed; and that, when the hand is introduced, more or less of what is called spasmodic contraction of the organ is found. That the so-called spasmodic contractions do nearly always attend such cases, is proved by the experience of Dr. Burns. After describing the circular bands of contracted fibres, found after labor, at the upper part of the cervix, and similar "bands higher up, grasping the placenta," which he properly thinks "are very inaptly called hour-glass contractions," he says: "If I did not know the hazard of establishing a general rule, I would say that in almost every instance this contraction takes place. I have rarely introduced the hand, in a case of flooding, without meeting with it, whether the placenta had or had not been expelled."

The importance of these facts is obvious; and if the point for which I contend—that placental adhesion is the cause of hour-glass contractions—be established by the observation of others, the subject will be placed before the profession in a clear and certain light. The source of the frequently-occurring post-partum hæmorrhage which is the cause of so much anxiety to the obstetric practitioner, will then, except in purely anæmic cases, admit of ready explanation. And when experience has proved that the fear of injuring the womb by the introduction of the hand is groundless, a prompt removal of the after-birth will insure safety to the patient in nearly every case.

I could refer to a number of cases of partial placental adhesion to illustrate the position I have taken, but shall confine myself to the most recent. On the 7th of March last, at 1 P. M., I was called to see Mrs. F., of New York, in labor with her first child. Age, 24 years; health, good; pelvis, well formed; temperament, sanguineous; pains, regular and effective; os uteri, open to the size of a quarter of a dollar; fœtal pulsation, distinct; placental *soufflet*, at the posterior wall of the uterus. Ten P. M., the os uteri relaxed, and two inches in diameter, the pains regular, and every indication of a favorable result. At five P. M. the 8th, sixteen hours after commencement of labor, the pains were very severe, frequent, and irregular; the os uteri was well open, and relaxed; the head was down upon the perineum, but did not advance with each pain. Mental excitement ensued. She declared it was impossible to endure the pains. Chloroform was used, but with only temporary effect. When not under the influence of the remedy she would rise, and throwing herself forward upon the bed,

would endeavor to press the uterine tumor against its edge. Yet the pulse did not increase in strength and frequency. She was with difficulty prevailed upon to bear the pains patiently until medicine could be procured. I gave her 25 min. of tinc. opii., ¼ gr. of tar. antim., and ℥ss. of nit. ether, every hour, until the desired effect was produced. She was soon composed, and rested, with occasional sleep, for nine hours. At 3 P. M. the labor-pains returned, and in three hours she was delivered; thirty-one hours from the commencement of labor.

During the last three hours of this labor the placental *soufflet* was unchanged, as it had been from the beginning, and the uterine tumor was irregular in form, and thrown more forward than usual. At the moment of delivery, a quantity of blood was thrown off from the vagina. A hand was placed upon the abdomen to excite the fundal muscles, but without effect. I waited two or three minutes, but as flooding continued, and as I had reason to think that the placenta was adhering, I introduced the hand, and found a firm semicircular contraction opposite to the placenta. The upper part of that body adhered to the uterine surface; the lower portion was thrown off, and from that part of the organ the blood was flowing upon my fingers. When the adhering portion of the after-birth was peeled off, and the hand carried to the fundus, its circular fibres contracted, the hæmorrhage ceased, and the whole mass was removed.

In this case about one-third of the placenta, the part near the fundus, was adhering firmly to the wall of the uterus. The other two-thirds must have been separated before delivery, for the labor at its close was rapid; there was very little pulsation in the cord; the child was perfectly blanched, threw out blood from its mouth, and lived only ten hours. The muscular fibres of the anterior and lateral walls of the womb were found in a state of firm contraction. Did this contraction take place during the two or three minutes that elapsed between the birth of the child and the introduction of the hand? Or was it the result of the contractile power of those muscles slowly acting upon the child during the process of parturition, and upon the placenta also, grasping it until a part was forced from the uterine surface? That the muscles of the posterior wall of the organ, the place of the placental attachment, had not contracted, so as to aid in effecting delivery, is evident from the fact that they had not constringed the uterine blood-vessels at that part. That the head of the child was impelled through the pelvis by a "vis a tergo," is equally evident; and that force could be no other than the nervi-motor power

of the unrestrained muscular fibres, which were found after delivery in the contracted state that they would naturally assume in accomplishing the end which, in a physiological labor, would have been effected by the muscles of the fundus.

During the last few days I have met with a case of placental adhesion and irregular uterine contractions similar to the above, but attended by very peculiar circumstances. On the 21st of April I was called to see Mrs. L., residing in New York, the mother of seven children. She was at the close of her sixth month of pregnancy; aged 39 years, with good general health, but large from extreme obesity. Three days before I saw her she had taken a very long walk. I found her suffering severely with marked symptoms of enteritis, and occasionally pains, which she described as "bearing down like after-pains."

For the first forty-eight hours I treated her for the enteritis, during which time the uterine contractions continued. At 9 A. M. on the 24th, I called in Dr. Joseph S. Perry to see her. As there were no unusual circumstances attending the labor, our attention was directed principally to the bowels. At 3 P. M., the os uteri being well open, I ruptured the membranes, and seven hours after delivered her of a living fœtus, which gave one faint cry, and expired in a few minutes. After-pains, with flooding, followed. I endeavored for ten or twelve minutes, but in vain, to deliver the placenta by gentle traction on the cord, and friction, with pressure upon the fundal muscles. Two fingers were then passed into the vagina, and a portion of the after-birth was found hanging loosely, just within the os uteri. Four fingers were then introduced into the womb, when they came in contact with a circular band of contracted uterine fibres, which retained the upper half of the placenta in a chamber above; while the lower half was separated from the posterior wall of the organ, from which blood was flowing.

As I had never introduced the entire hand into the uterus at six months, I hesitated. But when I considered her illness for the three previous days, her lingering labor, her great prostration, the relaxed os uteri, and the copious flooding, I decided to operate. The hand was then cautiously introduced into the uterus. The points of the fingers passed up over a portion of adhering placenta until they reached the apex of a cone formed by the irregularly-contracted muscles of the fundus, while the back of the hand was grasped by the contracted fibres. The adhering part was then carefully removed by beginning at its upper point, the right hand being placed at the same time upon

the abdomen. The contracting fundal muscles then followed the hand, and effected delivery. During her recovery I could discover no evidences of unusual uterine irritation.

Comments upon this case are unnecessary, for every physician can decide for himself whether or not the tonic contractions were the result of irregular uterine action during parturition, induced by placental adhesion. I shall also leave it to the members of the profession to judge as to the propriety of removing the placenta by the introduction of the hand into the uterus at so early a period of pregnancy; while, at the same time, I would not hesitate to repeat the operation under similar circumstances.

As to the time when the placenta should be removed in these cases—a matter of vital importance—we find that most of the European obstetric practitioners still cling to the old rule, that is, "to wait." Hardy and McClintock, the latest writers on the subject, state that Dr. Robert Lee gives only one hour, but says it is usual to wait two hours. They also, in common with most other transatlantic accoucheurs, have a dread of doing injury to the uterus by introducing the hand into the body of the organ. For they say: "Of all the causes necessitating the artificial removal of the placenta, its morbid adhesion is, perhaps, the most unfavorable." In this country, however, such views are not now generally entertained, and many physicians have formed an opposite opinion, and adopted a very different practice. Dr. Meigs says, in his Treatise on Midwifery, that "the indication of the hour-glass contraction is, *perhaps*, to deliver at once," and advises "the student to separate as soon as he can clearly determine its existence." And I may venture to assert that the usual practice in the city of New York is to remove an adherent placenta as soon after delivery as circumstances will possibly permit. All that is wanted in such abnormal conditions of the uterus is a physiological action of the circular fibres at the fundus, and the contraction of the organ to a small hard ball at the pelvis. But in cases of flooding, caused by placental adhesion, it is useless to excite irritation upon the surface from which the blood is flowing, unless it be at the fundus; to employ force upon the contracted band, with the view of relaxing the fibres, is worse than useless, for it is replete with danger. When the circular fundal fibres are made to act by gentle friction upon them, with the hand grasping the afterbirth, these difficulties, so much dreaded by accoucheurs, will vanish.

Whatever may have been the experience of others, I have not found any difficulties to attend or to follow the *immediate removal of the placenta* in such cases, although I have adopted the practice for a

number of years. In Mrs. G's case, notwithstanding the exhaustion that followed her labor of 57 hours, her uterus contracted to the shape and position mentioned above, and continued in a normal state throughout her recovery.

The following practical inferences may be drawn from the above facts and views:

First. As it is the duty of the attending physician in every protracted labor to ascertain the cause of delay, he should know, if possible, whether there be placental adhesion or not. I find it can be diagnosticated by auscultation, and by attention to the form of the uterine tumor, and to the action of the circular fundal muscles. If, after repeated trials, made as the labor progresses, no change can be perceived in the distinctness and loudness of the placental *soufflet;* if the muscles of the fundus do not act at the beginning of each pain, and contract regularly throughout its continuance; if, during pain, the uterine tumor does not present to the manipulator a globular form, but is more prominent than usual at the place of placental attachment; if these symptoms be accompanied by irregular and inefficient pains; if there be a relaxation of the os uteri, in the absence of pelvic or other difficulties; and if the labor be lingering, it will be safe to conclude that the placenta is adhering to the walls of the uterus.

Second. The danger from hæmorrhage, which attends almost every labor with placental adhesion, calls for the most careful and constant attention on the part of the practitioner. Nothing but a prompt removal of the whole mass, and a firm contraction of the womb, will insure the safety of the patient. If, therefore, there be any symptoms during a labor that indicate a firm connection of the whole or a part of the after-birth, it should be detached after delivery, and removed without waiting, whether there be visible hæmorrhage or not. Expelling pains will only add to the irregular contractions, and increase the difficulty of its artificial removal.

Anti-partum hæmorrhage may also occur. For one or more lobules may morbidly adhere, while the others are thrown off by the contractile power of the free uterine muscles, in which case urgent symptoms would soon demand a speedy instrumental delivery. Ergot is totally inadmissible in this emergency, because it would increase the abnormal contractions, without having any power over the fibres surrounding the patulous mouths of the bleeding blood-vessels.

Third. When it has been satisfactorily ascertained that a labor is attended by placental adhesion, not complicated with other difficulties, the safest practice to mother and child, in most cases, is to depend

upon the "vis medicatrix naturæ." If, however, there be good reason to believe that the labor is protracted *solely* from the want of regular fundal and other uterine contractions, consequent upon such an adhesion, the life of the child may be preserved, and the safety of the mother more effectually secured, by a timely and cautious use of the vectis, or of one or both blades of the forceps; provided the condition of the os uteri and the position of the head are favorable.

May, 1858.

Case of Diphtheritic Inflammation of the Kidneys—Uræmia—Leucæmia—Obliteration of the Femoral Veins—Abscess of the Knee-Joint, and Purulent Infiltration of the Thigh. By Professor Louis Bauer, M.D., Member of the Royal College of Surgeons, England, Licentiate of the State Medical Society, etc.

Ernst S., a German tailor, thirty years of age, was brought, on the 2d of April, 1859, in an unconscious condition, into the Long Island College Hospital, and placed under my charge.

Little information, as to the history of his case, could be collected. The patient had been in his usual feeble health, when he was suddenly attacked with facial erysipelas, which affection had been of moderate extent and character. But very soon a low delirium set in, which of late, however, had become periodically so violent and troublesome, as to render his removal to a public institution imperative.

On clinical examination, the following conditions were recorded: S. is a man of medium size, with a frail and delicate constitution. Various cicatrices connected with the bones of his right foot and leg, as well as a deformity of the former, evince previous suffering of the patient from *caries*.

The erysipelas of his face is passing off, and desquamation has already commenced. The color of his skin, and especially that of his face, is unusually pallid, with a yellowish tint. The temperature of his body is rather lowered than otherwise; pulse 105, (4 o'clock, P. M.,) and rather small, feeble, and soft. The action of the heart almost normal, although slightly enfeebled, with the exception of a moderate blowing sound accompanying the systole. Head cool, and no signs of congestion; sclerotica of the eyeballs exceedingly pallid, and the expression of the eyes glaring; pupils readily reacting; mucous membranes of nostrils and mouth, pale and moist; tongue clean, and the patient indifferent as to food or drink. Abdominal cavity neither sensitive nor distended with gas; urine retained, and the bladder distended. There is some sensitiveness over the lumbar region. Respi-

ration moderately accelerated, otherwise normal. Vital powers prostrated, with marked muscular tremor, and a passive and apathic position. The urine being withdrawn by catheter and subjected to a careful chemico-microscopical examination, revealed a moderate quantity of albumen, and but few traces of urates, besides epithelial and diphtheritic casts, blood-corpuscles, and numerous solitary epithelial cells with fat granules.

These clinical facts rendered the diagnosis both clear and easy, which was set down as diphtheritic inflammation of both kidneys, with a moderate degree of uræmia, and the result of the treatment verified in every respect my opinion. My prescription consisted of the administration of carbonate of soda, and cups upon the lumbar region.

During the subsequent night, the patient enjoyed but little rest; he was delirious, but not violently so. Towards morning, he had a few hours' sleep, from which he awoke rather improved. The febrile excitement and the muscular tremor had evidently lessened, and the sensorium was less obscured. There is still albumen in the urine, with the microscopical elements before mentioned.

I should have previously stated, that the patient's domestic habits had been every way temperate and orderly, and that, therefore, his case could not have been set down as delirium tremens, which, in some respects, it obviously resembled.

Treatment continued, and light but nutritious diet allowed.

April 4.—Patient has rested well; sensorium almost free, and muscular tremor decidedly lessened. Pulse, 88. No soreness of any account over the kidneys; albumen in urine decreasing. State of vital powers low. No other changes. Treatment as before.

In this ratio the patient progressed steadily up to the 11th of April, so as to be fairly considered convalescent. About this time, however, a new phase commenced. After an unusually restless night, the patient complained on the 12th of April of soreness and swelling of the left knee-joint. When examined, the joint was discolored, the synovial sac distended by a considerable quantity of liquid, and was painful both on touch and motion.

The reaction of this new trouble upon the system was but moderate, and the coexisting febrile excitement mild. This was evidently an abscess of the articular cavity. Its remote causes could not be ascertained at the time, despite a most searching examination. When the abscess formed, the patient was in a fair way of recovery, and all his vital functions were in a comparatively good order. He was, moreover, under the use of iron and a generous diet, both of which told

favorably upon the anæmic state of his blood. My next suspicion was directed to the blood-vessels of the interested extremity, but neither phlebitis nor obstructions by thrombus or embolus could be discovered; nor did the patient complain of pain, tension, or uneasiness, beyond the anatomical limits of the joint itself. No other remote cause for the formation of the articular abscess seemed to be feasible than *spontaneous pyæmia*, for which, indeed, the vitiated hæmatosis of the patient gave the best pretence. At any rate, the general character of the disease was duly acknowledged, and tonic, restorative treatment and diet were systematically observed. Mindful of the old surgical principle, "*Ubi pus, ibi evacua*," a principle that has gained in strength by the very latest microscopical research as to the proximate generative sources of pus, I decided upon the withdrawal of the articular contents. The operation was performed upon the *cul-de-sac* of the synovial membrane, above the external condyle of the femur, the extremity being previously extended, and the matter pressed towards the point of puncture. Thus, the matter was removed from the articular cavity without air having the slightest chance to enter.

Its quantity amounted to nine fluid ounces, and was not homogeneously mixed, but consisted in part of pus, and in a large proportion in perfectly transparent synovial fluid. The knee-joint was then surrounded with adhesive straps, and the entire extremity safely placed in a straight iron splint, well padded with cotton.

That the pus was clearly of recent origin, was subsequently ascertained by the microscope, exhibiting, as it did, numerous and very large mother-cells, besides solitary pus and epithelial cells and granules. Moreover, it evinced no smell or other signs of decomposition.

This proceeding relieved the patient materially, and secured to him for the four subsequent days rest and comfort. The joint, however, having rapidly filled again, had then to be punctured a second time, when a large quantity of purulent and homogeneous matter was withdrawn. Its quality was mild, without disagreeable odor, and of the ordinary microscopical elements. On motion, the joint manifested no crepitus. I had not discovered until this time a firm cord accompanying the femoral artery from the knee-joint upward to the inguinal glands. The artery, however, pulsated throughout its entire course. The inguinal glands commenced just then to swell, and to indurate. The large saphena vein was perfectly permeable, and to all appearances intact. The forementioned cord seemed to have a width of almost an inch, was painless, firm, and unyielding on pressure. According to the anatomical position of the cord, it could be either one

or both femoral veins, or the lymphatic vessels accompanying the femoral artery, but the size seemed to exclude the probability of the lymphatic vessels being inflamed and obstructed. The conclusion I arrived at, after a careful analysis of all the circumstances, was a simple thrombosis of the femoral veins, with interrupted circulation through their respective courses.

But few days had elapsed when a general purulent infiltration throughout the entire thigh and part of the leg, with œdematous swelling of the corresponding foot, appeared, the former requiring free incisions.

At this juncture two pathological questions offered themselves:

1. What is the present condition of the blood of our patient, and has it furnished the cause for the existing thrombosis in the femoral veins; or,

2. Have the other organs, concerned in the hæmatosis, participated in the morbid process?

Two questions of great moment, and equally difficult to solve. But for the highly important inquiries and experiments of Virchow, Julius Vogel, and others, our knowledge on these points would have been truly meagre, since we have, at least, some clue to the pathological condition of the blood in general; and however inefficient my humble efforts may prove to be, they may nevertheless tend to affirm facts already ascertained, or instigate more interest for these subtle pathological points.

It cannot surprise us that the patient, already in a most enfeebled condition when admitted, should have become still more debilitated under so copious a drain upon his constitution, aside from other functional disturbances. His appearance was indeed ghastly, all his color having completely vanished. Such intense pallor I had observed but once—in a young man, poisoned with iodine—even not after the most profuse hæmorrhages. This pallor of itself indicated a considerable reduction of the red blood-corpuscles, and a numerical preponderance of the white cells. In order to ascertain the relative proportions of those microscopical elements of the blood, I had a few drops of blood abstracted. By mistake, the House Physician had chosen the affected extremity. The blood exhibiting under the microscope almost exclusively white, granulated, and nucleated cells, rather larger than pus-cells, and more like the white blood-corpuscles, my colleague, Dr. Daniel Ayres, very appropriately remarked, that that examination could not be considered conclusive, as long as the possibility existed that the parenchymatous blood might have accidentally come in contact with

pus. I directed, for the purpose of obtaining a positive result, a cup to be applied over the cardiac region, and a sufficient quantity of blood to be withdrawn for examination. Thus, about 3 drachms of blood were procured and received in perfectly clean vessels.

In connection with this operation, it was stated that the blood had oozed out rather more freely and rapidly than under ordinary circumstances, and continued so even after the cup had been removed, so as to render a styptic necessary.

The fresh blood was then beaten, in order to separate the fibrin, and was then put aside for settling. When examined about an hour after, three distinct layers could be recognized, consisting respectively of fibrin, serum, and hæmatin.

The fibrin presented, on a gross comparison, the ordinary quantity and microscopical appearance, though rather soft, and mixed up with numerous white blood-globules. The serum exhibited numerous yellowish flakes, an immense number of extremely delicate fat-granules, (soluble both in oil of turpentine and sulphuric ether,) and other elementary molecules of a rather indefinite character; moreover, epithelial cells and beautifully-developed white blood-corpuscles, in reversed* proportion to the red ones. In fact, I saw no more than four red blood-corpuscles in any one object; nor did any of my colleagues of the medical staff of the institution.

The serum was of neutral reaction. Crystals of salts were not obtained by evaporation. When boiled, the serum coagulated readily. The sediment was of a very high and almost carmine color; it consisted of the very same microscopical elements as the serum, and exhibited *scarcely a trace* of red corpuscles, whilst the liquid presented a yellowish hue. At this place I should mention that the blood withdrawn from the left extremity differed in some of its microscopical characteristics. Firstly, its color was of a brown-red, and rather darker; its fibrin was softer, and it exhibited but an imperfect coagulation, and had more white globules than the other specimen. From these clinical and microscopical examinations, the fact of leucæmia, or the abnormal preponderance of the white blood-corpuscles, was positively established.

As ordinary causes of this disease of the blood, profuse hæmorrhages, suppuration, and more especially tumescence of spleen or liver, have been mentioned; the latter in connection with inveterate inter-

* According to Donders, Moleschott, and Kölliker, five white corpuscles to two thousand red ones is the proper proportion in normal blood.

mittent fever. Hæmorrhage as a cause had to be excluded, since the patient had never been subject to copious loss of blood. But it is not unlikely that the caries, and the attending copious discharges of purulent matter, from which he had suffered in previous years, had laid the foundation of this vitiated condition of the blood, and that the subsequent scrofulous derangements of the lymphatic and glandular system had both propagated and perpetuated the disease. For the patient clearly states that his general health had always been feeble, and his color unusually pallid.

Although the patient had never suffered from intermittent fever, yet I instituted a most careful investigation into the state of his abdominal and thoracic organs, but without finding the least noticeable morbid alterations. The liver, though slightly sensitive, seemed to be of ordinary size and quality, and the spleen, if anything but normal, was somewhat lessened in size. The lungs presented the ordinary respiratory sounds and resonance, and if there were tubercular deposits at all, they were certainly of small extent, and of no pathological account, perhaps already calcariarized. The heart-sounds were almost normal, though the systole manifested a rough, blowing sound, indicating, perhaps, small fibrinous fragments upon the semilunar valves, frequently observed in leucæmia.

As to the pathological dependence of the various morbid changes, no positive theory can be advanced. It seems, however, feasible to me, that the leucæmic condition of the blood had been the pre-existing disease of late, engrossed by the uræmic poisoning, and that the thrombosis was an accidental occurrence, chiefly resting on the increased coagulability of the fibrin of the blood.

It will be readily perceived that the case of Ernst S. bears a remarkable conformity with that form of pyæmia hitherto called spontaneous pyæmia or purulent diathesis; and but for our better understanding of the physiology of the blood, I would have been obliged to resort to the same expedient diagnosis.

As yet we possess no means, either chemical or microscopical, of discriminating between mucous-cells, white blood-cells, and pus-cells, they being almost of the same size, organization, and chemical susceptibilities. But evidently the whole mass of blood could not consist of pus-cells, and at the same time sustain life. This is obvious.

The formation of the so-called spontaneous or metastatic abscess will probably be taken by some surgeons as an evidence of pyæmia, which pathological interpretation is the only one hitherto considered feasible and legitimate. The physiological and pathological researches of the

last two years have, however, changed our pathological comprehensions, so as to revolutionize older doctrines entirely, and to replace them by new ideas, having at least the merit of a positive demonstrative basis. I should exceed by far the limits of this article were I to enter fully upon the discussion of this highly important pathological subject; but thus much I may say, that the formation of spontaneous lobular and metastatic abscess is now satisfactorily explained by Virchow's discovery of the pathological effects of thrombosis and emboli. Nor is the rapidity with which metastatic abscesses form any longer incomprehensible, since the anatomico-physiological matrix of pus has been clearly ascertained. Even the sudden purulent effusions in serous cavities have been brought within the scope of our anatomical and pathological understanding by the observations of Heidenhain. This subject being of great moment, I hope to find sufficient leisure to discuss its merits and bearing in this journal at an early time. The time, indeed, may not be far when pyæmia will be entirely exploded, as untenable and devoid of real existence. This, however, is a mere inference, drawn from the evident tendencies of the latest pathological discoveries.

I return after this digression to the patient.

May 4.—The condition of the patient is changed in but few points. His debility has materially increased; temperature higher; skin dry; pulse 136, small, weak and empty. The action of the heart greatly agitated, and the blowing sound aforesaid stronger and more distinct. Appetite and rest satisfactory; abdomen tympanitic, and inclined to constipation. Stools exceedingly fœtid, and rather pale, as if deficient in cholèviridin. Urine highly concentrated, but without direct morbid elements. From the left thigh there is a fœtid, decomposed, and discolored sanious discharge of considerable quantity, containing rags of mortified cellular tissue. The obliterated femoral veins present the same condition. At this time, the blood being again microscopically examined, some change for the better was noticed. The number of red corpuscles had considerably increased, though still below normality; they exhibited, however, a crenated and wrinkled appearance, disappearing on the action of water, and moreover they evinced little tendency to join each other in the usual manner.

As to treatment, I need hardly state that the best and most concentrated food, with wine, beer and milk, and tonic remedies, were liberally resorted to, which were also continued by my calleague, Dr. John Byrne, in whose hands the patient subsequently passed.

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As to treatment, I need hardly state that the best and most concentrated food, with wine, beer and milk, and tonic remedies, were liberally resorted to, which were also continued by my calleague, Dr. John Byrne, in whose hands the patient subsequently passed.

By his kind permission, I had, a few days ago, another opportunity

of seeing and examining my former patient, who is not only still alive, but somewhat improved. The blood still contains about fifty per cent. of exceedingly pale-white corpuscles, which, however, could be clearly discriminated when a solution of iodine had been added. The suppuration still exists, but the discharge is limited to the thigh and to a small place on the leg; it is decidedly improved in quality and lessened in quantity. The venous cord is still the same, and it seems as if an obliteration of the femoral veins had been effected. The venous circulation of the extremity seems to be tolerably free, and to be obtained in a collateral way.

Although the case of Ernst S. has been brought before the profession in a rather fragmentary form, yet its scientific interest, independent of its termination, is sufficiently strong to warrant a full exposition. Bearing as it does upon the present investigation of pyæmia and the various pathologic conditions of the blood, and being a case in point, it will in some respects elucidate the subject and aid in its final settlement.

BROOKLYN, *June* 15, 1859.

SUPPLEMENT.

Since the foregoing clinical history was written, the patient has gradually sunk, and finally died, on the 6th inst. The post-mortem examination was made a few hours after death. The body was almost reduced to a skeleton, the fat and muscles being wasted. The color of the skin was pallid, with a dirty yellowish tint, indicating the highly cachectic nature of the disease. There was a large decubitus on the sacrum, exposing the bone itself to some extent. About the left knee-joint there were some fistulous openings, penetrating severally the articular cavity; and some other openings leading to large fistulous tracts along the thigh.

From the diagnosis I had been induced to form, my attention was chiefly directed to the condition of the femoral vessels, and to all those large parenchymatous organs which, according to experience, are so frequently the seat of lobular abscess. I commenced, therefore, with the left thigh. In tracing the course of those vessels with the fingers, I could still discriminate the cord which I have previously adverted to. It had the thickness of almost an inch, and was still firm and even. The adjacent parts, although thoroughly infiltrated and comparatively dense, were much softer than the cord.

I then proceeded to the cord in its full length; both vessels and the femoral nerve were found to be converted into almost a homogeneous

mass, of dense structure. The constituting parts were then dissected; the artery was found passable, and the veins, although evidently compressed, exhibited no signs of inflammation or thrombosis. The femoral nerve was exceedingly firm—almost hard. Next I traced the femoral vein upward into the abdominal cavity, when a thrombus of two inches in length was met with in the left iliac vein. The thrombus extended a short distance into the hypogastric vein, and was riding on its angle of bifurcation, being, however, not thick enough to fill the lumen of the vessel; nor was it so firmly adherent to its walls, or of so firm a consistence, as to be considered an ante-mortem formation. My diagnosis of thrombus of the femoral vein was therefore not verified.

In the popliteal space, the inguinal region, the pelvic cavity, and up to the spinal column, numerous lymphatic glands were found, and all of them in a high state of inflammatory engorgement, from which it seems that an inflammation of the lymphatics had been going on. Whether this inflammation is to be looked upon as the commencing and primary element of the disease, or whether the common sheath of the vessels was the primary structure involved, I am at a loss to determine. Evidently, however, an inflammation had been started in both these tissues, and by its products caused such restraints as to interfere seriously with the circulation through the femoral vein; whilst the artery, by virtue of its elastic coats, had to a certain degree been exempt from pressure, although its lumen had evidently been reduced, which was ascertained by comparison.

My next attention was bestowed upon liver and spleen. The former was considerably enlarged, greatly engorged, and in an incipient state of fatty disorganization; but no abscess, even of the smallest size, could be discovered, though I cut it in small pieces, that no part could escape my search. Its general condition recognized as inflammatory hypertrophy; by which I, however, wish to express the general increase of its size, in part attributable to the fatty material, whilst I am uncertain with reference to the increase of its essential anatomical elements.

The spleen was of a dark-blue color, its capsule detaching itself readily, and in a state of advanced softening, though of normal size.

Both kidneys were considerably enlarged; their capsules tore off readily; the cortical substance much congested, besides moderately pale, and granulated; whilst the medullary substance was of a deep-brown discoloration.

Under the microscope, fatty degeneration of the various epithelial

cells, partly detached, partly forming cylinders, was observed. The remaining abdominal organs presented no morbid changes.

The right lung was perfectly healthy, whereas its pleural sac was filled with inflammatory products, and the entire pleura inflamed and covered with plastic material, evidently of recent origin. The left lung presented in its apex tuberculous deposits in an advanced state of softening; and in the left side of the heart were the pathological evidences of endocarditis, the tunica intima being opaque, whitish, thickened, and covered with closely-adhering plastic material; whilst in the right side a large, though soft, fibrinous clot was found, extending from the right ventricle both into the pulmonary artery and the right auricle.

This is the summary of the post-mortem examination, from which it appears that my original diagnosis, as to the affection of the kidneys, the liver, the heart, and more especially the femoral vein, was substantially correct. If there was no thrombus in the vein, there was at least a compression of its lumen, tantamount, almost, to its organic closure. But the pathological consequences were of course different. A thrombus in the femoral vein would have most probably led to an earlier fatal termination of the disease, by embolus into the parenchyma of the liver, and lobular abscess. It is difficult, indeed, to say whether the leucæmia originated with or produced the morbid changes of either or both—liver and spleen. Virchow states, however, that tumefactions of these organs are the ordinary concomitants of leucæmia; and though this question has not, as yet, been clearly settled, yet he inclines to the supposition of their being the *causa movens* of that disease.

If the case of Ernst S. has not been as perfectly elaborated as its pathological importance and interest certainly demanded, and if some links are wanting of the latest history of his case, it is due to my resignation from the Long Island College Hospital, and the transfer of the patient to another medical attendant. A material loss, however, has not been caused thereby, as the history of his case comprises the greater and more important portion of his disease, and the post-mortem condition.

July 16th, 1859.

Report of Cases of Diseases of Bone occurring in Bellevue Hospital, N. Y. By J. M. FARRINGTON, M.D., Trumansburg, N. Y., late House Surgeon.

1. *Caries of the Skull.*—S. E., æt. 35, was admitted in hospital May 22d, 1858. In 1849, he was poisoned with syphilis; had chancres and indurated glands, which did not suppurate, for which he received no treatment, save local applications. About a year afterwards had a swelled testicle, which again was treated only locally. In the course of a few months had ulcerations upon the legs, then took some internal remedies; but is confident no mercurials. A few months later had ulcerations in the throat, from which he recovered under the use of iodide of potassium. A year later, had severe frontal headache, usually commencing at 3 o'clock, P. M., and continuing not more than two hours; and at the same time had nocturnal pains in the tibiæ. After six or seven months, more suppuration was established in the left temple, and an exfoliation of a piece of the left parietal bone took place. After the lapse of twelve months more, a piece of equal size, viz., one inch in diameter, was exfoliated from the posterior portion of the same bone. After the complete separation of the above-mentioned pieces of dead bone, these points of discharge healed perfectly; but they were succeeded by others, four in number, scattered over the vertex, and which remained open at the time of his admission. He had at this time a small ulcer upon the left arm, immense cicatrices upon either leg, and "soft spots" on his head, over the points where the bone had been lost. He was conscious of a marked failure in memory coming on since the trouble first appeared in the skull. The openings in the scalp communicated with extensive patches of roughened, dead bone, and on probing it carefully, it appeared as though nature was attempting the exfoliation of the external table of both parietal, and of a considerable portion of the frontal bones. The discharge from these openings was very fœtid and profuse; they were dressed with pledgets of lint, saturated in a solution of chloride of soda. Tonics and anti-syphilitic remedies were ordered. On the 3d of July he was operated upon by Dr. Wood, visiting surgeon. The patient was put under the influence of chloroform. An incision was made around the forehead high up, from temple to temple; this incision was met in the mesial line by a longitudinal incision, made over the vertex, and the scalp was dissected up, so as to expose the whole front of the calvaria, and dead bone was chiseled off from over a space two inches square; the bone was much thickened; was not at all loose, and therefore much more difficult of removal than had been anticipated; the wound

in the scalp was closed by silver sutures. A great amount of suppuration naturally occurred from the under portion of the scalp, before it became again adherent to the skull. The extent of the disease was such, that Dr. Wood did not deem it prudent to remove the dead bone at the sides of the head at the first operation. The patient passed from under my observation before the second operation, and I am not able to give the result.

2. *Necrosis of the Os Frontis.*—J. G., æt. 66, of good constitution, but of intemperate habits, was admitted in hospital October 13th, 1857. He had always enjoyed good health, and had never had syphilis. When fourteen years of age, he fell from a horse, and struck the left side of his forehead against a stone, causing a small wound through the integument, which soon healed, and he had no further trouble from it. About four years since he was run over by a team of horses attached to a sleigh. His head was considerably bruised, and he was rendered insensible for a few moments. In a few days he fully recovered from the effects of the injury, and had no further trouble therefrom. One morning in the summer of 1855, on awaking, he found that his right eye was blackened, though not swollen; but from this time it began to swell, and at the end of about ten days suppuration was established, and the abscess was opened by an incision made into the upper lid, near the inner angle, and just beneath the orbital arch. A large amount of matter was discharged through this opening, and it had never closed; matter had continued to be discharged, and the upper lid had become retracted and adherent at this point. The fistulous opening was characteristic of diseased bone. Four weeks previous to admission another abscess formed upon the centre of the forehead, which abscess was opened, and had continued to discharge ever since. He was put upon the use of tonics at the time of his admission, to improve his general health; under which treatment he continued until the 24th, at which date Dr. Wood cut down upon the diseased bone at the site of the superior opening, and removed a small portion of dead bone. Tonics, cod-liver oil, and good diet were ordered, and the upper opening eventually closed; but the discharge from the lower opening still continued. On the 23d of January, 1858, Dr. Wood operated upon this portion of the bone. An incision was first made over the orbit, and the integument reflected; the trephine was then applied over the frontal sinus, and on removing the button of bone, an abscess was discovered beneath, which communicated with the fistulous opening above the eye. The patient was under the influence of anæsthetics.

A slight inflammatory blush surrounded the eye, and extended over the forehead for several days after the operation. Tinct. ferri, chloridi, and quinia were administered, and the parts were dressed with a lotion of acetate of lead and opium. The sinus was dressed daily, with pieces of lint, in such a manner as to give the discharge free exit. The lower opening eventually closed; and the operation, novel, and important in its character, was considered very successful. In May he was attacked with erysipelas, and on recovering from it, he asked his discharge, fearing that, if he remained, he would have another attack when the disease should be prevalent in the hospital. He left the hospital on the 19th of May, and at that time the discharge from the opening made by the trephine had not entirely ceased, though it bid fair to eventually do so. Both fistulous openings, as before stated, were closed, each operation having been followed by success in removing the diseased bone, the cause of the discharge.

3. *Necrosis of Inferior Maxilla.*—Margaret M., æt. 29, came into the surgical wards March 25th, 1858. About six months previous she suffered from pain in one of the molar teeth of the right side, accompanied with pain and swelling of the lower jaw on that side. The pain in the tooth soon subsided; but the painful and swollen condition of the jaw continued until her admission into the hospital.

About four months previous, an opening was made, and matter discharged externally; this opening and discharge continued permanent.

Thre were two cloacæ, one above and the other below the jaw-bone. The lower opening was large enough to admit the end of the finger. There were also other cloacæ communicating with the buccal cavity; giving, by means of the fœtid discharge from the decomposing bone, an intensely ill odor to the patient's breath, to correct which, a gargle of a weak solution of chloride of soda was directed to be used several times a day. Her general health was good, and consequently no medication was resorted to. On the 10th of April the affected portion of the jaw, comprising about one-third of it, was removed by Dr. Wood. An incision was first made from the angle to the symphysis of the jaw, the integument dissected up slightly, when the dead bone was readily seized by a pair of strong forceps, and detached in fragments. Separation of the periosteum was resorted to by Dr. Wood, previous to the removal of the bone. No anæsthetic was used; the patient did not suffer much; only one artery was divided of sufficient importance to require a ligature. The edges of the incision were brought together by sutures and adhesive straps. The

upper cloaca was also closed by sutures; but the lower and larger one having too ragged edges to admit of good coaptation, was left open, and thus formed an outlet to the discharge from the wound. The parts were dressed with dry lint at first, but in consequence of the offensive odor of the discharge, the dressings were subsequently dipped in a creasote lotion, or solution of chloride of soda. Two days after the operation, the silken sutures which closed the upper cloaca had ulcerated through, and the silver wire suture was substituted; but in consequence of the tension required, this also came out by ulceration; the opening, however, soon after closed by granulation. On the first of May, all the external wounds, save the larger cloaca, were entirely healed.

By constant pressure of the tissues outward, by the lint on the inner side of the mouth, and by the rapid process of granulation, the tendency to a sunken deformity was remedied.

The edges of the cloaca were firmly attached to the bone, and thus prevented a successful result in our efforts to close it. Sutures, straps, and vivification of the edges were resorted to, until the opening was so small as just to admit a probe, and forming a salivary fistula. This existed at the time she took her discharge.

There was a new ossific deposit in the place of the bone that had been removed, which deposit preserved the outline of her features almost perfectly.

4. *Necrosis of Scapula.*—Sarah E., æt. 24, was admitted in July, 1858, for necrosis of the left scapula. The diseased portion occupied the upper margin and angle. She was operated upon by Dr. Stephen Smith, visiting surgeon, who successfully detached and removed the necrosed margin of the bone, and the patient made a good recovery.

5. *Caries of the Ribs.*—This case was a man aged 40. The disease was of syphilitic origin. He did not remain a sufficient length of time under treatment to make his case worth reporting. It is mentioned here merely to show the comparative frequency of disease of different bones, so far as it may be illustrated in the cases that occurred in the second surgical division of Bellevue Hospital during a period of ten months.

6. *Caries of Carpal and Metacarpal Bones — Amputation of Hand.*—John C., æt. 55, was first admitted in hospital August 26th, 1857, for disease of the bones of the right hand. Three years previous, while cutting wood, he received a contusion of this hand. He stated that there was an abrasion of the skin, which soon healed, but was succeeded by a small, dark-colored swelling, which soon became

of the size of a goose-egg. The tumor, he said, was thoroughly cauterized, and the hand afterwards passed under various forms of treatment, continuing up to the date of his presenting himself at the hospital. At this time the dorsum of the hand was an open abscess, and there was much swelling of the part involved.

Caries of the metacarpal bones of the index and middle fingers existed. He was put upon the usual treatment for his disease, and remained in the hospital until the 14th of December, without any radical improvement. There was a considerable amount of discharge from the diseased parts, and his general health was evidently deteriorating. At the above date, a surgical consultation was held by the visiting staff, and it was decided that the local disease ought to be removed by amputation of the hand; to this the patient would not consent, and he was discharged. Unable to sustain himself, he went to the Almshouse, on Blackwell's Island.

On the 16th of March, 1858, while he was an inmate of the Almshouse, spontaneous and profuse hæmorrhage from the hand occurred, and it was with difficulty controlled only at the end of twenty-four hours, and after having twice reoccurred. He sought admission to Bellevue Hospital the second time, and now he wished the hand removed. His appearance was much worse than when he left. The loss of blood which he had sustained had probably much to do with his blanched appearance. He was more emaciated than when discharged, and quite feeble. On the 3d of April, Dr. Smith performed the circular operation, just above the wrist-joint, sawing off the lower end of the ulna and radius, to avoid the synovial secretion from their articular surfaces affecting the result. Ether and chloroform were used. The stump was dressed daily, and went on doing pretty well until the 8th, when the roughened edge of the ulna, which had been causing ulcerative absorption of the tissue covering it, protruded. Care was taken from the first to avoid such a result, but it was inevitable. No union by adhesion took place, and the pus was unhealthy in character. Stimulants were given, and nutritious diet. The ligature on the radial artery came away on the 11th. The sutures were removed on the 7th. The last ligature came away on the 16th. On the 28th, the end of the stump was closed, excepting a small point in the centre. On the 5th of May, the edge of the extremity of the ulna, which had protruded posteriorly, was exfoliated, it being a mere scale of bone. On the 18th of May the stump was entirely healed, and the patient was walking about, enjoying good health.

7, 8, 9, 10, 11, were cases of *Caries of the Vertebræ*. Only two

of these cases remained under treatment a sufficient length of time to show the effects of medication. One of these was not benefited; the other, after many months' treatment, was certainly, though very slowly, improving. She still remained in the hospital at the close of my service, (Oct. 1st, 1858,) and with the hope of an eventual cure of her infirmity. Her case was a peculiarly aggravated one; the treatment was varied, and yet decidedly tonic and anti-strumous. Wine and extra diet were allowed. Dr. H. G. Davis, of N. Y., kindly furnished his apparatus for the treatment of this disease, and applied it to this case. The patient was too feeble and the back too tender at the time for her to wear it long, and it was removed. Its object was to give the spinal column sufficient support, so that she might be able to sit up. She may yet be able to wear it. She was also put upon a pill of nitrate of silver and nux vomica, (as suggested by Dr. Davis,) from which she appeared to derive benefit. The lower dorsal vertebræ were the seat of the disease.

12. *Necrosis of Femur.*—D. S., æt. 28, was admitted Dec. 19th, 1857. Sixteen years ago his knee was injured by the rolling of a barrel of meat against it. He continued to work for six weeks after the injury, at the end of which time it was much swollen and painful at a point just above the knee. It was poulticed until suppuration was evident, and then opened at two points; one just above the knee, the other half way up the thigh, on its posterior aspect. He was laid up for four months before he was able to attend to business, since which time, however, his leg gave him no further trouble, until about three months prior to his admission, at which time he fell down a flight of stairs, striking upon the same knee. Pain and swelling of the thigh followed. Suppuration took place, and the swelling opened spontaneously at about the same points as those above mentioned. On probing, denuded bone could be felt at the upper point. He was put upon the use of iron and vegetable tonics. Free exit was given to the discharge by tents of lint, and poultice applied. The lower opening was soon induced to heal, and the discharge from the superior one was much lessened when he left the hospital; the limb was devoid of pain, and he felt well enough to go to work. The amount of disease was so small and deep seated in this case, and gave the patient so little inconvenience, that an operation was not deemed advisable.

13. *Necrosis of Tibia.*—B. K., æt. 22, was admitted in hospital Oct. 29th, 1857. When but four years old she had a fall and fractured both bones of her left leg, near the ankle. It was about two years before she entirely recovered from this injury, and she had no

further trouble with the limb until eight weeks previous to her admission, when, in stepping from a car, her foot turned and she wrenched the left ankle. She was a domestic, and continued at her work for two weeks after the accident, at the end of which time the ankle was much swollen and very painful, and she was forced to relinquish her household duties and keep in bed. A physician was called, who directed the application of six leeches to the joint, to be followed by poultices. Shortly afterwards the swelling opened spontaneously, and discharged a large amount of blood and matter. At the time she came in the hospital there were four openings in the ankle: one just above the external malleolus, and three on the inner side; these were the chief ones, and they communicated with diseased bone. On the 7th of Nov., Dr. Wood laid open the inner side of the leg, just above the ankle, exposing the diseased tibia. By the use of trephine and chisels the dead bone was separated; quite a mass was removed. The cavity left was stuffed with lint after the operation, and this was the only dressing applied, excepting water, until the wound began to suppurate, when the lint was removed and replaced by new as often as was necessary. After a few days the dressings were saturated with a lotion of creasote and water, (℥i. ad Oij.) Abscesses formed from time to time about the wound, which retarded the progress of cure. As there was much tissue to be restored by granulation, the process of reparation was necessarily a protracted one. In two months after the operation the wound was nearly closed or filled by granulation. She was able to walk about the wards in January, but remained in the hospital until the 23d of March. At first walking had the effect of inducing renewed congestion and pain of the parts affected; but this gradually subsided, so that when she took her discharge she could walk about well, and there was but a very little tenderness remaining.

14. *Necrosis of Tibia.*—M. P., æt. 16, came into the surgical wards January 7th, 1858. He had necrosis of the right tibia. Seven years before a barrel of beef rolled over him, and wounded his right leg midway between the knee and ankle. The tibia was exposed for a considerable distance, but the flap of removed tissue was strapped down over the bone, and it healed kindly in about a month, excepting a small opening over the bone, which continued to discharge for some ten weeks, at which time it healed, and he had no further trouble from it. For the past five years he had worked in a cotton-mill in a country village. During each summer, while there, he had been accustomed to be much in the water bathing; often six times in one day. In August, 1857, he was attacked with pain in the right leg and ankle;

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this increased, and an abscess soon formed, which opened spontaneously, and had continued open and discharging ever since, and several small pieces of bone had from time to time been exfoliated. The bone was enlarged at the time of his admission, and presented evidence of exostosis. There were three openings in the integument, communicating with the disease beneath. The diseased portion of bone extended from a point two inches above the inner malleolus to nearly the middle portion of the tibia. On the 16th, Dr. Wood removed the sequestrum; the hæmorrhage was considerable; chloroform and ether were administered, and, after the operation, stimulants and anodynes. The treatment of this case subsequently was similar to case 13. On the 1st of April the cavity was nearly filled by granulation, and the bone was no longer exposed. He remained in the hospital until the 19th of May. He was able to walk about well, and was considered cured, though there was still a slight discharge from the part.

Case of Congenital Hernia of the Funis. By C. K. BRIDDON, M.D., Surgeon to the New York Dispensary, &c.

(Read before the N. Y. Pathological Society, Sept. 14, 1859.)

On the 9th of July, 1859, I saw the male child of Hugh and Mary O'Connor, in company with Drs. Pulling, Aigner, and William Corson. The patient was three days old, and was ushered into existence by a midwife. It presented a large globular dilatation of the funis. (See cut.) The expansion commenced immediately at the insertion of the cord into the abdominal walls; it measured two and a half inches in its transverse diameter, and nearly the same in its long axis; the circumference of the narrowest part or neck of the tumor, at the junction of the funicular tunics with the abdominal integuments, was nearly four inches; the skin around the umbilical aperture had been drawn down by the weight of the protrusion, forming

an investment to its pedicle about half an inch long; the dilated funis evidently contained a portion of the hollow viscera of the abdomen, conveying impulses from that cavity, and pressure forcing air audibly from the incarcerated contents into the same.

The taxis was perseveringly tried, but without avail, and it was determined to expose and reduce the contents. The necessity for such a step was arrived at after mature deliberation, and on the following grounds: The expanded cord was in process of desiccation; in a few days its separation would expose its imprisoned contents, such process resulting either in adhesion of the intestine to the abdominal opening, with slough of the protrusion and the formation of a fæcal fistula, or fatal peritonitis would be the issue. The existing condition and probable results having been explained to the parents, they desired that any means might be used that would avert the formation of an unnatural outlet.

Operation.—An incision in the direction of the long axis was made through the funicular investment of the tumor; the dissection was carried carefully through all the tissues, until the intestine was exposed. It was impossible to detect any sac; the mass of contents were at every point adherent to the inside of the expanded funis. These adhesions were easily broken down, and the whole mass was liberated from its covering; but this simply exposed the contents, and further proceedings were necessary to enable us to reduce them. The whole had a truly formidable appearance: it had plainly been incarcerated for some time; it had none of the features of normal intestine; the surface of the folds were covered with organized lymph; the sulci, or involutions between the different folds, were filled up, level with the surface, with solid semi-transparent lymph. The whole was gradually and carefully unfolded, the adhesions yielding readily, and on examination it proved to be the cœcum, a portion of the ascending colon, and a large portion of the ileum. It was now found that the opening through which this had passed was too small to permit its return, so thickened and solidified had the intestinal coats become during their sojourn in the cord; the margin was notched on the right side to avoid the vessels, and the cutting instrument was guarded by the finger. A little gentle dilatation enlarged the opening sufficiently to admit of return of the gut, and a strong ligature, embracing the neck of integuments which surrounded the pedicle of the tumor, effectually closed the external opening. Over this a compress and bandage was applied, and the little patient was put to bed. The child did not appear to suffer much from such apparently harsh proceedings; the hæmor-

rhage was not great, and came solely from vessels ramifying in the plastic material thrown out on the surface of, and in, the interstices of the mass.

10th.—The child has been restless and cross through the night; the abdomen is slightly tympanitic; the bandage was removed, and tr. opii camph., gtts. ii., were ordered every hour.

11th.—Condition much the same as yesterday. Abdomen more distended, and resonant; the child does not take the breast, and vomits green mucosities; has had no evacuation by the rectum.

12th.—The patient remained in much the same state until night, when it died. Just before death it had several watery stools.

13th.—Necroscopic examination 11 hours after death. The integuments are tinged of a yellow hue, abdomen tensely distended, and covered with large tortuous veins. The abdomen was opened by two elliptical incisions inclosing the umbilicus; the inflated intestines bulged through the incisions, and at one point were found adherent to the ellipsis of skin removed in exposing the abdominal cavity. There was no effusion into the peritoneal sac, and the traces of inflammatory action were limited to those portions of the intestine that had been incarcerated in the funis; the descending, transverse, and a portion of the ascending colon, were found healthy, empty, and contracted; at about the middle of the ascending colon was a well-marked contraction of the calibre of the gut, and immediately on the proximal side of this the intestine expanded, and was altered in structure. The expansion was not associated with thinning of the parietes of the tube; on the contrary, they were thickened in some situations to the extent of two and two and a half lines. This condition existed in the cœcum, and eighteen inches of the ileum, where it ceased less abruptly than at the seat of obstruction. The peritoneal surfaces of these portions of the tube were maroon-colored, in some places purple, in others darker, approaching black; but they were not in a condition of sphacelus, were not softened, and did not give way at any part in the manipulation of examination. The mucous surface was less intense in color, but still highly congested, and its epithelium easily detached. The portions described were those that had formed the hernial tumor; the remaining portions of the intestine, the upper portion of the ileum, the jejunum, duodenum, and stomach, were greatly distended with aeriform and fluid contents.

Therapeutic Uses of Glonoine or Nitro-Glycerine.

(From the French, for the Monthly.)

Glonoine is a compound formed by the action of nitric acid on glycerine. An English physician—Field—made the first experiments on this substance, which were afterwards extended by Baker Edwards to the whole series of xyloid substances, such as xyloidine, pyroxyline, saccharoine, glonoine, benzoine, etc. All these substances should have, according to the last experimenter, an action on the nervous system, more or less resembling that produced by strychnine. Glonoine was thought to be the most active of all these substances; in dose of 20 drops it was reported to have produced effects, in a vigorous adult rabbit, resembling, in a remarkable manner, those determined by strychnine. Field and Brady claimed that glonoine would be efficacious in cases of neuralgic pains, doses almost homœopathic being simply required. Fuller and Harley, on the other hand, have recognized in this substance, when given even in very large doses, a much less potent influence than is attributed to it.

In consequence of such contradictory statements, Mons. Vulpian has repeated the experiments of Fuller and Harley, and they have furnished results which demonstrate that glonoine does not produce any of the effects of strychnine, and is not poisonous ordinarily, even when given in moderately large doses. A young rabbit, that had been sick for some days, and very much emaciated, was made to swallow from 80 to 90 centigrammes of pure glonoine. Two and a half hours after, there was no perceptible effect; but it died in 18 hours, without exhibiting any convulsion, at least during the last hours. In other experiments no effect at all was produced. Thus a young dog, of large size, swallowed the same dose (80 to 90 centigrammes) of pure glonoine, without any sickness. The same dog, some days after, swallowed, without any inconvenience, four grammes of pure glonoine, and, after an interval of a few days, took four grammes more, two of which were dissolved in alcohol, and yet there were no morbid phenomena.

Looking, on the one hand, at the almost negative results of experiments on animals with large doses of glonoine; and, on the other, at the report of the English physicians, claiming that phenomena, very manifest and sometimes very serious, have been produced in man after the ingestion of a drop of a solution containing one per cent. of this substance, it is proper to ask whether the same substance has been employed in the different experiments, or if errors have not occurred in the consideration of the results obtained. In the presence of

such contradictions, we can only remain in doubt, until new and more complete experiments shall allow us to decide on what side rests truth.—*Revue Thérap. du Midi*, June 30. L. H. S.

The Curative and Prophylactic Action of Bromine, with Reference to Pseudo-membranous Affections. By Dr. OZANAM.

A. *Mode of Employment.*—The activity of this substance, its volatility and ready decomposition, make it very difficult to be employed medicinally. Alcohol, ether, and chloroform are solvents. They combine readily with the metaloid, but they form a complex product of bromo-formate of ether, bromhydric acid, and bromine; and it is no more a matter of indifference to administer bromhydric acid for bromine, than to employ sulphureted hydrogen instead of sulphur, or chlorohydric acid for chlorine. These solvents should be rejected, as excipients. Distilled water dissolves bromine without decomposition, and without oxydation when protected from light. Our best treatises on chemistry, however, are content with the mere statement that bromine is slightly soluble in water, without mentioning the proportion. Glover fixes the per centage as 1/41,23 in 100 parts of water, at 62°F. Ozanam finds, however, that the degree of solubility varies very much according to the agitation or succussion employed, and the temperature.

When the temperature is low, and the water is poured on without any violent agitation, the bromine is taken up in very small proportion—about 1/12000. But when the temperature is high, and considerable agitation is employed, the solution is much greater, having been as much as 1.50 parts at 15° C.

In view of this great range of solubility in a medicine so energetic, it is important to have some strict pharmaceutical rules for its preparation. Ozanam employs bromine water containing one-thousandth of bromine, as both readily tolerated by the stomach, and sufficiently active. The solution is prepared by shaking 10 centigrammes, or 2 drops of pure bromine, along with 100 grammes of distilled water, and agitating until the solution is complete. The result is a liquid of a beautiful amber color, perfectly transparent, and with a characteristic odor. Kept in the dark in a ground glass-stopped bottle, this will preserve for a long time its amber color and its unalterability, especially if the bottle is full. But if this preparation is to be preserved for a long time, or to be transported, it is better to add to the bromine

an equal quantity of bromide of potassium, which will render it much more stable, and prevent its becoming acid.

The following formula then is recommended:

Potassii bromid.,	0.10 centigrammes.
Brom. pur.,	0.10 "
Aquæ distil.,	100 grammes.

Bromine water is spoiled by free exposure to the air, especially when there has been no addition of bromide of potassium. Indeed, the bromine, on account of its volatibility, tends constantly to escape in the form of vapor; the liquid discolorizes gradually, beginning in the upper layers; and, if the bottle is without a stopper, the bromine completely disappears in a few days.

Bromine water should be cautiously administered in cases of croup and kindred diseases, beginning with very small doses, one to five drops a day, in water or gum julep, to be taken by spoonfuls. If the stomach tolerates the medicine without difficulty, the dose can be increased according to the age of the patient and the violence of the attack. It has been given in certain cases to the extent of from 25 to 30 drops a day, but this cannot be exceeded without provoking gastric pains and vomitings.

Bromine is also soluble in *glycerine*, and the solution is preserved a longer time, when exposed to the air, without decomposition; but when exposed to light, acidity is quickly developed.

B. *Prophylactic Virtues of Bromine.*—Ozanam thinks that he has protected entire families by requiring every member employed with the patient to take from 3 to 6 drops daily in sugar-water. Whether the morbid influence was only experienced in *malaise* and sore throat, or there was threatening of the disease, with redness of the throat and whitish spots already apparent, the danger has always been removed.

A more serious and difficult case led him to discover the anti-miasmatic value of bromine vapors. He was called during the month of May, 1858, to see a young girl, in a boarding-school, attacked with decided croup. She was treated by bromine water, and cured, after having thrown off at different times pseudo-membranous tubes, from 10 to 12 centimetres in length. The infirmarians, who nursed her night and day, also took bromine, and escaped. Some days after three other children in the same house were attacked; they were isolated, and placed under treatment. Five days afterwards three new cases occurred in a class which had no communication with the chamber containing the sick. He could not conceive the cause of this epidemic until he learned that numerous similar cases were under treatment at the *Hô-*

pital des Enfants, and that the wards in this hospital appropriate to such diseases were those nearest to the boarding-school. It was, then, the air which brought these miasms, and hence it was necessary to purify and protect the whole house.

He placed in each dormitory, and in the infirmary, glasses filled with water, in which three or four drops of pure bromine were poured; this substance is so diffusible, that in a short time the atmosphere was impregnated with it. The vessels were removed in the evening, so as to avoid any injurious effects the vapors might exercise in the dormitories at night. This plan was successful; all the sick got well, there were no new cases, and he had the pleasure to arrest the germ of a contagion that might have been most dangerous in a house containing nearly one hundred persons. The fumigations were continued for more than twenty-five days, so as to prevent every chance of return.

This valuable property of bromine can often be made useful in hospitals. The article is employed in such small quantity, on account of its energy and diffusibility, that a small bottle will suffice to purify large halls for weeks, and to preserve them from a serious epidemic.—*Gazette des Hôpitaux*, May 24, 1859. L. H. S.

Monthly Summary of Cotemporary Medical Journalism. By O. C. GIBBS, M.D., Frewsburg, N. Y.

Iodide of Zinc in Chronic Conjunctivitis.—In the *Peninsular and Independent*, for August, Dr. Waggoner, of Oconee, Ill., has a short article upon the treatment of chronic conjunctivitis. After correcting the general health, if deranged, he advises the application of one-sixteenth of a grain of sulph. morphine, dissolved in water and applied with a camel's-hair brush, twice a day, until all irritability is allayed. After this follows the curative treatment, which does not materially differ from that usually put in requisition, except that advised in his concluding remarks. He says, "Latterly I have incorporated in my materia medica the *iodide* of *zinc*, the effects of which have proved satisfactory beyond all anticipation. This drug is passed over in all our standard works on therapeutics, in almost profound silence. In no case, in which I have observed its effects in the treatment of ophthalmia, has it deserved, in my humble opinion, a place second to any other remedy. In one scrofulous case, it acted like a charm. Will not the profession give attention to this very deserving agent, and

more fully prove its worth?" The Dr. does not give his formula for use.

Congenital Hernia.—In the same number of the *Peninsular and Independent*, Prof. Moses Gunn, of Detroit, reports a case of hernia, descending through a congenital passage for the first time after the age of 30 years. If this case is not *unique*, such cases must be of rare occurrence.

Miasmata.—In the *Lancet and Observer*, for August, Dr. Harvey, of Springtown, Indiana, publishes a paper upon the above ever vexatious subject. It is difficult to give a synopsis of the paper that will be sufficiently condensed to answer our purpose. He says, "Those diseases originating from miasmatic causes are produced by a change in the relative proportion of the several gaseous and mineral elements of which the living body is composed; in other words, diseases which appear both epidemically and sporadically, and some others, are produced by the addition to, or subtraction from, the essential elements of the body in health." Again, he says, "In intermittent, bilious, and remittent fevers, the crassamentum of the blood is increased, and darker in appearance than in health. The red globules are darker, and the quantity of serum less than the healthy standard; the fibrin having been either changed to albumen or coagulated to a certain extent. Hence we may infer that there has been *carbonaceous* matter from external sources added in undue proportion, or a decomposition within the body." Want of space will not permit us to give the premises upon which this idea is based. He adds further, "In such diseases as cholera, dysentery, &c., we must look for other causes; but these may be found in other gaseous or mineral elements or compounds, emanating from similar sources as carbon, and also from the earth. My opinion is, that cholera is produced by the action of *nitrous acid gas* upon the blood. In certain electrical conditions of the atmosphere, the oxygen and nitrogen of the air become chemically united, and form nitrous oxide or protoxide of nitrogen; and these two gases, being disengaged from decaying animal matter, become united also, with the same result; and the resulting compound floats in the air, and is absorbed through the lungs, skin, food, &c., into the system. I believe that the great amount of moisture in the air, the great heat and presence of electricity everywhere in those countries where cholera is raging, causes a greater or less chemical union of the elements of the air, thereby producing nitrous acid gas."

The miasmatic element, as productive of disease, and of varying diseases, has been an incomprehensible mystery and vexation to ob-

serving physicians for all time, and is likely to be for some time in the future. Some have denied its existence altogether, as an aerial poison, the product of animal or vegetable decomposition, and as causative of endemic or epidemic diseases. We will here instance only S. Littell, M.D., of Philadelphia, who refers such diseases to electrical agencies, and the late J. K. Mitchell, M.D., who referred them to a cryptogamous origin. That certain diseases occur only when there is a conjunction of certain atmospheric and terrene conditions, is almost universally admitted; that the terrene conditions exercise a controlling influence, is a fact too well established to admit of doubt; and that that terrene condition or element is effluvial, is more than problematical. But why dissimilar diseases from similar causes? That the plague and yellow fever occur only under similar, if not absolutely identical conditions, is well known. High temperature, humid atmosphere, lowness of site, density of population, and animal and vegetable putrescence, with a preponderance of the former, are necessary conditions to the production of a miasm that will develop either of the above-mentioned diseases. If the causes are the same, why not the results? If these conditions develop the plague in Constantinople, why not in New Orleans; and the reverse, in regard to yellow fever? We answer by putting another question. If sugar, starch, and gum-arabic are composed of precisely the same ingredients, in two of the instances, in precisely the same proportions, why are not the physical properties of the resulting compounds identical? If dissimilar substances are isomeric, may not the causes of dissimilar diseases be also isomeric?

Intermittent, remittent, and bilious remittent fevers have also terrene causative relations and dependencies, and those causatives are apparently identical, and always present where these fevers are endemic. The miasm of intermittent differs from the miasm of yellow fever and cholera, but we are not prepared to believe the difference to be what Dr. Harvey suggests. The miasm of an intermittent has a more vegetable origin than that of yellow fever, and its conditions of development are not the same. If intermittent fever has always terrene causative relations, why is not that disease always present whenever those causative relations exist? True to our Yankee instincts, we answer by asking, Why carbon and hydrogen, in the relative proportion of five parts of the former to four of the latter, in combination, are not always oil of turpentine, and not sometimes oil of lemons, as is well known to be the case? Why is not $C_{12} H_{10} O_{10}$ not always starch, when in combination, and not sometimes gum-arabic and gum-tragacanth respectively?

This is not the time or place to enter upon controversial ground—the above thoughts are thrown out simply as suggestives.

Laryngismus Stridulus.—In the same number of the *Lancet and Observer*, Dr. R. R. McMeens, of Sandusky, Ohio, reports a case of the above disease, with a few remarks upon its pathology. Dr. Hord, in the *British and Foreign Medico-Chirurgical Review*, declares the exciting and sustaining cause of the disease to depend upon an enlarged state and disordered function of the liver—a condition of that organ vulgarly termed and treated by nurses as "livergrown," and the laryngeal difficulty, dyspnœa and spasm of the glottis, are sympathetically excited and secondarily involved; and offers, in support of the allegation, the revelations of twelve autopsies of fatal cases, all confirmatory of the fact. "To the truth of this proposition," Dr. McMeens says, "I am disposed to defer, from the ostensible efficacy of remedies adapted to such pathological inductions. Cathartic doses of calomel, followed by free discharges of vitiated bilious secretions, were invariably succeeded by a marked amendment, while the absence or deficiency of such secretion was precursor to a decided aggravation of the disease. As an alterative, the proto-iodide of mercury had a most happy influence over the secretions. Dover powder usually sufficed to allay any disturbance or pain in the bowels. Frictions along the spinal column were premised for the contraction and insensibility of the extremities, and the inhalation of ether, hyoscyamus, and anise, to soothe the irritability of the larynx and allay cough. Under this treatment, at least, the disease was manifestly modified, and the patient steadfastly improved.

"How such a pathological condition and disordered function of the liver is capable of producing such an inveterate affection, and involving so distant an organ, by sympathetic influence and nervous concentration alone, is a question of no easy solution, and would require the talent and ingenuity of a Henry Frazer Campbell to clearly elucidate."

The pathology of this affection has been anything but definitely settled, and anything concerning it we regard with interest. Some, with Drs. Ryland and North, have supposed the disease dependent upon cerebral derangement, while others consider it dependent upon gastric irritation. Dr. Marsh thinks it may proceed from irritation of the pneumo-gastric nerve; while Dr. Ley supposes it dependent upon paralysis of the muscles which open the glottis, in consequence of pressure of enlarged glands upon the recurrent nerves. It is probable that the laryngeal affection is due to a reflex nervous

action, the seat of irritation being variously located: sometimes in the gums; at others, in the medulla oblongata; and still at others, in the liver, or any part of the digestive apparatus.

Gun-shot Wound through the Abdomen.—In the August number of the *New York Monthly Review*, Dr. J. J. Edmunds, of Buffalo, reports a case under the above heading. He says: "On examination, we found two wounds; one in the centre of the right gluteal region, and the other in the back, nearly opposite the junction of the lumbar with the dorsal vertebræ, about three inches to the left of the spine. The ball had passed through the bowels, wounded the left kidney, and lodged in the abdominal muscles, about two inches above and to the left of the umbilicus." The patient recovered. Dr. Edmunds seems positive that the intestines were wounded in this case, and seems to think that, in similar cases of recovery, surgeons unwarrantably suppose "that the ball either passed round, without entering the peritoneal cavity, or in some manner glided between the intestines, without inflicting injury." Had fæcal matter escaped by the wound, the Dr. would have had proof positive that the intestines were perforated; but, lacking that evidence, we must be permitted to doubt. If any evidence for our opinion is wanted, we beg leave to refer to the report. On the eighth day of the treatment, with six grains of morphine daily, the bowels moved spontaneously, "*and quite natural.*" Had the intestines been riddled with the ball, we must be permitted to think that the evacuations would not have been *quite natural*, and the result, we may suppose, might have been different.

Removal of the Toe-nail.—In the same number of the *New York Monthly Review*, Prof. Mack, of Buffalo, in his Review of Parisian Medicine and Surgery, says, "In-growing toe-nail is removed by M. Gourut by the following process: Short strips of adhesive plaster are placed on one another, so as to form a kind of brim; two such fasciculi are made, and placed a little in front and a little behind the morbidly-growing nail. Into the groove thus artificially made, and which just occupies the root of the nail, semi-fluid Vienna paste is put. After a few minutes, a black eschar is formed, properly hemmed and limited by the adhesive plaster. The paste is now quickly taken off; and in a few days the nail comes off, without pain, by the gentlest traction.

A Substitute for the Ecraseur.—In the *Chicago Medical Journal* for August, Prof. Daniel Brainard figures, and describes a blunt scissors, invented by himself, for the removal of vascular tumors, when the *écraseur* could not be applied. It is adapted for the removal of tumors of the throat, and he reports one case in illustration of its utility. A

vascular tumor of the fauces was successfully removed, of which Dr. Brainard says, "Its removal would have been very difficult without the forceps, as the knife allowed too much bleeding, and the *écraseur* could scarcely be applied."

A New Method for preventing Hæmorrhage in Operations on the Lips.—In the same number of the *Chicago Medical Journal*, Prof. Brainard figures a pair of forceps for compressing the facial artery in operations upon the lips. The forceps are bent at the point, the object of which is to turn the handle out of the operator's way, and they are fixed by a slide. He says: "This, with slight additional pressure by the fingers of an assistant of the alæ nasi, in cases of hare-lip, or upon the chin in operations for cancer of the lower lip, so effectually arrests the hæmorrhage that chloroform may be safely used. These forceps are convenient for holding the lip while the needles are being inserted." In all operations for hare-lip, and for the removal of small tumors of the same parts, we have arrested hæmorrhage with the heated wire. It is speedily applied, and quite effectual.

On the Simultaneous Existence of two Diseases Originating from Specific Morbid Poisons.—In violation of the programme which we had marked out for ourselves—that of confining our *Summary* to American Medical Journal Literature—we make reference to an article, by Charles Murchison, M.D., of London, upon the above subject, published in the July number of the *British and Foreign Medico-Chirurgical Review*. It has been a prevalent opinion in the profession that two specific diseases could not run their course in the same system at the same time. Dr. Murchison introduces cases to show that two morbid actions and two eruptive diseases can coexist. For instance, typhus fever has been known to run its course with typhoid, and also with scarlet fever and erysipelas; hooping-cough with measles, small-pox, and the vaccine disease; small-pox with scarlet fever, measles, erysipelas, vaccine disease, &c.

These statements all may be true, and the cases well observed and correctly reported; but we have vaccinated many a time, after an exposure to small-pox poison, and have never yet seen variola and vaccinia running their course at the same time. In one instance, (for report of case see *American Journal of Medical Sciences*, vol. 24, p. 566,) we vaccinated a patient four days after an exposure to small-pox, and three days thereafter, thinking the vaccination was going to fail of effect, we performed variolous inoculation. The vaccine disease was produced, with no peculiarities of intensity or prolongation of development; the variolous exposure and inoculation proving abortive.

Erysipelas.—In the *Medical and Surgical Reporter* for August 13th, J. R. McClurg, M.D., of Philadelphia, has an able article upon the above subject. In regard to treatment, after the action of an emetic and a saline cathartic, combined with a mercurial, if there be symptoms of bilious derangement, he says: "I always use some tonic medicine, and my favorite prescription, and the one I uniformly use in all cases of erysipelas, of whatever form or character, as soon as the system is prepared for it, is,

℞.—Spiritus ætheris nitrici, f. ℥ij.
Tinctura ferri sesqui chloridi, f. ℥ij.
Quiniæ sulphatis, gr. xvj.

Misce et S. Take a tea-spoonful every three hours. This constitutional treatment I have found very successful in my hands, and desire nothing better."

This corresponds very nearly to the treatment we have found very serviceable. We are, however, in the habit of prescribing the tincture of iron in a little larger doses, say twenty drops every three hours; and the quinine in combination with Dover's Powders, from one to two grains of the former to five of the latter, every four or six hours.

A Case of Epilepsy Cured.—In the same journal for August 20th, Dr. S. N. Pierce, of Cedar Falls, Iowa, reports a case of epilepsy, cured under treatment in compliance with the following prescription:

℞.—Ext. stramonii.
" conii, ää gr. xv.
Strychnia, (cry.,) gr. ij.
Argent. nitr., ℈ij.
et div. in pil. No. xxx.

"Of these," he says, "I gave three a day. This course was pursued perseveringly, the amount of strychnia and nitrate of silver being slowly increased until I found the disease gradually yielding to its influence. I now have the satisfaction of feeling that the disease is subdued." Patients have recovered in cases of epilepsy under a variety of treatment that subsequent experience has proved to be of non-effect.

One case is quite too limited to judge of the effects of any medicine or combination of medicines. The case of Dr. Pierce loses in interest when it is remembered that the improvement at the time of the report was of but five weeks' duration.

Mental Influence on the Products of Conception.—In the *Medical and Surgical Reporter* for August 27th, Dr. Ziegler, of Philadelphia, reports a case of a mother who, in the early weeks of pregnancy, was badly frightened by what she supposed to be one of her children cut-

ting off the hand of another. She was confined at full time of a child minus the right hand—the radius and ulna terminating abruptly in a stump at the wrist. The editor adds that, four years since, he saw a woman delivered of a child with a deformed nose, which was in exact resemblance to that of an unfortunate girl's who lived next door, who had a cancer of the nose.

The first case was doubtless a case of spontaneous intra-uterine amputation, caused by the cord being wound around the wrist. The second was doubtless a case of incomplete development, which occasionally occur as freaks of nature, of which hare-lip is a sample. We once saw a nasal deformity similar to the above, but the lip was cleft. We hope our women are not going to reproduce all the deformities they see.

Strychnia in Chronic Intermittents.—In the *New Orleans Medical News and Hospital Gazette*, Dr. Harrison, of Arkansas, has an article upon the use of strychnia in chronic intermittents.

The following is his formula:

"℞.—Strychnia,	gr. iss.	
Sulph. Quinine,	gr. xv.	
Capsicum,	gr. vj.	
Brandy,	℥iv.	M.

"Of this mixture, I direct one tea-spoonful (for an adult) every hour, for six or seven hours preceding the expected paroxysm; at the end of this time I require the patient to take a cup of warm sage tea, and go to bed, (if he is not already there,) and remain until the paroxysmal hours pass. This course is to be repeated on the next 'chill day,' after which a tea-spoonful of the medicine is to be taken two or three times per day, until the four ounces are exhausted."

With Dr. H. F. Campbell's views of the nature of this disease, and of its relation to the nervous system, the philosophy of this treatment becomes at once apparent. This, however, is not altogether new treatment. Dr. Brainard, of Chicago, recommended strychnine, in an eighth-of-a-grain dose, three times a day, in similar cases, more than twelve years ago. (See *Indiana Medical Journal* for July, 1847.)

We are confident that the remedial powers of strychnia are not yet fully brought out. So far as we know, we were the first to use and recommend it, in *sciatica* and chronic rheumatism; and we have seen cases of dyspepsia and chronic costiveness yield to it like a charm.

Nature and Treatment of Cholera Infantum.—In the *Southern Medical and Surgical Journal* for August, Dr. Ford, of Atlanta,

Georgia, has an article upon the above subject, in which he advocates an unusual treatment. We have not space to give a full synopsis of his ideas. Suffice it to say, that he believes the disease to have malarious causative relations, with such *ganglionic* irritation as is conformatory to H. F. Campbell's idea, conveyed to vascular organs and secretory surfaces. We quote his treatment: "I recommend as a good plan, as far as my individual success is concerned, as well as scientific reasoning, the following treatment: Sulp. Quinine and sulp. iron, half grain of the former, and quarter of a grain of the latter, three times daily. Besides a mixture of this kind, I give sometimes three doses of the quinine, commencing early in the morning, giving one dose every two hours." "Lime-water, camphor-water, and bicarbonate soda were used freely, when there was excessive vomiting; also mush poultices with mustard, over the epigastric region." Dr. Ford speaks of combination of iron and quinine as though it was in solution, but he has given us no formula for such solution. He reports good success; but, confessing to a little skepticism, we are hardly prepared to abandon the old treatment.

Use of Alum on Bougies in Strictures.—Dr. Mason, of Richmond, Va., in the August number of the *Virginia Medical Journal*, reports an obstinate case of stricture that yielded promptly to the use of bougies dipped in pulv. alum. He says, "The very first attempt was successful, when every other admissible means had failed. What the rational of this treatment is, let others attempt to explain. In a few days we were able to introduce the largest-size catheter without using alum, the stricture having entirely yielded." Should subsequent experience confirm this report, a valuable addition will have been made to our means for the relief and cure of this painful difficulty.

Solution of Tannic Acid in Glycerine.—A "Retired Pharmacien," in the August number of the *Atlanta Medical and Surgical Journal*, says: "The remarkable solvent power of the glycerine for tannic acid may be resorted to with advantage, when it is desired to secure a powerful astringent or styptic effect.

℞.—Tannic Acid.
Glycerine, ää 1 ounce. Dissolve.

A bit of sponge, wet with the solution, and passed up to an ulcerated and bleeding os uteri, restrains the hæmorrhage satisfactorily. The various applications of a compound so powerfully astringent will readily suggest themselves; diluted with an equal bulk of water, it is used for hardening the nipples.

Pile Ointment.—℞.—Tannic Acid, 2 drachms.
Sulphate morphia, 6 grains.
Glycerine, 2 drachms.
Lard, 1 ounce.

Rub the acid, morphia, and glycerine together in a mortar, add lard, and mix thoroughly."

Sympathetic Vomiting in Pregnancy.—In the *Boston Medical and Surgical Journal*, for August 25th, Dr. E. D. Miller, of Dorchester, reports several cases of the above troublesome affection treated with local applications to the neck of the womb, and with prompt effect, and in some cases after the failure of other means. His application is the ethereal tincture of iodine, (from half to one drachm of iodine to an ounce of sulphuric ether.) This application is made once a day, or less frequently, as the case may require.

REVIEWS AND BIBLIOGRAPHY.

Urinary Deposits—their Diagnosis, Pathology, and Therapeutical Indications. By Golding Bird, M.D. Edited by Edmund L. Birkett, M.D. A New American, from the Fifth London Edition. Philadelphia: Blanchard & Lea.

Fifteen years ago, Dr. Golding Bird republished, in a single volume, a series of lectures, which had originally appeared in the *London Medical Gazette*, on Urinary Deposits. Few works on a special department of medicine were received with more general approbation than this, inasmuch as it embodied all the latest discoveries, and not a few of the original ideas and suggestions of the author himself, clearly explained and admirably illustrated. So well did it fulfill the wants of the medical profession, that in the short space of nine years it had gone through four large editions, each undergoing a republication in this country. Two years since, Dr. Bird having passed away from the scene of his labors, Dr. Edmund L. Birkett edited and brought forward a fifth edition, his object being, as he says, "to bring the work fully up to the present period, and at the same time, as far as possible, to preserve throughout his (Dr. Bird's) delicate touch." This work has been admirably republished by Blanchard & Lea, and we propose to examine briefly how far Dr. Birkett has fulfilled his promise.

There are few medical men who have entered at all into the subject

of Urinary Pathology, as it is termed, that are willing to recognize the views enunciated by Prout and Golding Bird in relation to the various urinary diatheses fifteen years ago, as true at the present day. The mistake they committed in attaching undue importance to an increase of some particular urinary element, and finding in this increase a special and independent morbid condition, is one that at the time they could scarcely have avoided. Nor is it for us of the present day, taught by the advances of science, and the labors of subsequent observers, to whom they in a measure gave the impetus, to decry the obligations we are under to them. There are few truths in medical science that have not been elaborated by slow degrees; and just as the text-books of yesterday have given place to those of to-day, so those of to-day will be superseded by others to-morrow. Therefore all honor and praise to Dr. Bird; he fulfilled his duty according to the light he possessed, and had he been spared to the profession, would undoubtedly have been now, as he was then, among the foremost in appreciating and teaching the views of the present day. This, we are sorry to find, Dr. Birkett has not altogether done. He still retains the old distinction into uric acid, phosphatic, and oxalic acid diatheses, although such a division can no longer be recognized as tenable.

Whether a deficiency or excess of uric acid be due, on the one hand, to an excess or deficiency of change in the urea; or, on the other, to too great or too little disintegration of particular tissues, matters not, so far as the question of diathesis goes. In either case, an excess or deficiency of this acid is the result of simple physiological changes, the causes of which may or may not be associated with disease. It is, therefore, but a single symptom, an important one oftentimes, it is true, but still it is not a disease, nor does it alone indicate a disease, nor by itself require a special treatment. Such, also, is the case with the phosphates; and as oxalate of lime is a healthy ingredient of the urine, as any one may satisfy himself by freezing a portion of his own, and observing that octohedra of oxalate of lime are sufficiently evident under the microscope; and as, moreover, the diathesis is deduced from the presence of a deposit merely, and not from any excess in the urine, we are forced to admit that there is as little ground for assuming it to exist as a special diathesis, as for the uric acid or the phosphates. In addition, every observer must have remarked how common large deposits of oxalate of lime are in perfectly healthy individuals, continuing oftentimes for years, increased usually, it is true, under attacks of dyspepsia, but still exerting no appreciable influence on the general health; and this view of

the pathology of oxalate of lime is held by Scherer, Lehmann, Bence Jones, and Owen Rees.

Laying aside this fundamental omission of Dr. Birkett's, much has been added in other respects. The first chapters, on preliminary details and analysis, are excellent. We are sorry that the editor has merely alluded to Liebig's mode of detecting urea by titrition, instead of giving a full and detailed account of the mode of performing it; for perhaps in no respect has so decided an improvement been effected in urinary analysis as in the estimation of the various elements of the urine by graduated and definite solutions. He has added, however, a valuable table for discovering the nature of urinary deposits by chemical reagents; and on page 66 there are some exceedingly well-timed remarks in reference to placing too much trust in chemical formulæ, as explaining vital phenomena; a plan too much relied upon at the present day, and one that may be made to prove almost anything.

He does not allude, however, in speaking of the physiology of uric acid, to Dr. Hammond's experiment, in which a black snake, whose urine contained no urea, was confined in a jar of oxygen gas, after which it exhibited it abundantly—an experiment so pertinent to the question under discussion, and which has been recopied from the original article more than once. He reiterates, also, the same accusation against Dr. Fricke that appeared in the second edition of the work, in which he is made to assert that the dumb-bell crystals, described then as oxalate, and now as oxalurate of lime, consisted of uric acid. Dr. Fricke, some years since, asserted that the dumb-bell crystals did not always consist of oxalate of lime, and stated that uric acid, in the process of disintegration, sometimes assumed this peculiar shape; and this fact is now abundantly recognized. No one doubts that the dumb-bells figured originally as oxalate of lime are such, but there is no proof whatever that they are oxalurate of lime, as Dr. Bird subsequently asserted, and as Dr. Birkett still continues to call them.

We must thank the latter, however, for his chapter and plates on renal tube casts, which is an important addition, but more might be added with advantage; and it is a little remarkable that, with the exception of the present work, there is no text-book for students that contains any information on this subject that is at all reliable.

The chapter devoted to saccharine urine is improved and added to, and contains now the latest views on this interesting, and as yet complex subject.

The final chapter, on the action of some diuretic agents, we have always considered as a masterly production, and one of the best evi-

dences of Dr. Bird's originality and talent. It remains nearly unaltered; but he left little or nothing unsaid, even as far as our present knowledge extends, and it was better that it should continue as it was originally.

We have thus glanced hastily at some of the more prominent faults and merits of the work before us. In spite of what we have said in its dispraise, it must still be the text-book for students until another shall be written, embodying the improved views of the present day, which as yet are distributed either in small monographs, or in works too abstruse and cumbersome for the mere medical student. F.

A Dictionary of Practical Medicine: Comprising General Pathology, the Nature and Treatment of Diseases, Morbid Structures, and the Disorders especially incidental to Climates, to the Sex, and to the different Epochs of Life. With numerous Prescriptions for the Medicines recommended; a Classification of Diseases according to Pathological Principles; a copious Bibliography, with References; and an Appendix of approved Formulæ: the whole forming a Library of Pathology and Practical Medicine, and a Digest of Medical Literature. By JAMES COPLAND, M.D., F.R.S.; Fellow of the Royal College of Physicians; Honorary Member of the Royal Academy of Sciences of Sweden; of the American Philosophical Society, and of the Royal Academy of Medicine of Belgium; lately President of the Royal Medical and Chirurgical Society of London, &c., &c. Edited, with Additions, by CHARLES A. LEE, A.M., M.D., Professor of Materia Medica and General Pathology in Geneva College, &c., &c. In 3 volumes; pp. 1,254, 1,101, 1,717. New York: Harper & Brothers, Publishers.

The present is an age of Cyclopædial literature. Works of this description are to be found in almost every department of art, learning, and literature; and the ponderous volumes before us are models of their kind. Really, they constitute a Cyclopædia of practical medicine. Each article is an exhaustive essay, condensing, with admirable skill and judgment, the combined wisdom of the present and past ages. We cannot too much admire the wisdom, varied talent, extended research, and patient labor which have culminated in the production of this work, which will ever remain as an enduring monument to perpetuate among the profession a grateful recollection of the author's name and fame. An analysis of the more than four thousand double-columned and closely-printed pages, in any justifiable space, is an impossibility, and a criticism we are not foolhardy enough to attempt.

We shall, however, essay to give our readers some idea of the character of the work, hoping, for the good of suffering humanity, that each one will see the necessity of placing the Dictionary upon his own table, and making Dr. Copland his daily consulting physician.

The *nature* of disease is fully developed, and the causes which bring about departures from a normal condition well brought out, under the heads of *disease* and *blood;* and again, with more special bearings, under the heads of *climate, endemic causes, epidemic influences, infections, contagions,* &c. The operation of these causes upon the vital forces, as manifested through the nervous and sanguineous systems, producing functional and structural alterations, is duly considered. The alterations of secretion, excretion, nutrition, and of structure, which supervene as the result of the operation of the *pathogenic states,* meet with a concise, yet thorough, enunciation. *Morbid anatomy,* so far as it has practical relations, receives due consideration under the heads of the several organs, systems, and tissues of the body, and the morbid changes observed by the microscope, as well as the unaided senses, in the simpler as well as the more complex tissues, receives as ample an elucidation as is consistent with the character of the work.

There is no work on practical medicine in the English language in which the principles of *general pathology* receives as full consideration as in the volumes before us; and every well-educated physician and thoughtful prescriber will readily admit that this subject forms the only true and legitimate basis for a successful practitioner of medicine. *Special pathology* also, under the heads of individual diseases, comprises all that is of practical import, developed by the recent investigations in morbid anatomy, physiological, general, and special pathology.

Intimately connected with the causes of disease, and the nature of the morbid conditions which characterize the various departures from health, are the *signs* and *symptoms* by which disease is manifested, and its peculiar character indicated to the observer. Dr. Copland's article on *symptomatology* is one of the best and most practical essays upon that subject that it has ever been our good fortune to read. Special symptoms, as characteristic of special disease, receive attention in their appropriate places. *Diagnosis* and *prognosis* are considered in connection with, and as related to, symptomatology.

Age, sex, race, climative influences, and epidemic constitutions, as connected with disease and therapeutics, are all considered with that amplitude which is consistent with the character of the work. Infections and pestilential diseases, epidemic fevers, and maladies of en-

demic origin, are all fully entered into, and views, harmonizing with the most reliable of recent opinions, are succinctly stated. "When discussing *fevers*—continued, exanthematous, malignant, pestilential, and puerperal—the author has endeavored to connect these with their causes, to describe the changes, both functional and organic, which successively occur in the fluids, general systems, and structures. He has divided the fevers which assume a continued type into the forms and varieties which most frequently appear in temperate climates, and as he has observed them. But he has contended that no precise line of demarcation can be drawn between several of them, one form passing, in individual cases, into the other the most nearly allied to it, according to the exciting cause, to the concurrence of other causes and determining influences, and to the existing epidemic constitution; certain forms thus prevailing, or predominating."* Under the head of *Pestilence*, the three great epidemics, cholera, yellow fever, and plague, are very ably considered, displaying the most familiar acquaintance with the existing literature of the subjects. Some opinions are here expressed which are not in accordance with our own; but, as they are enunciated with such ability, and defended with such an array of evidence, and as they are in conformity with those entertained by the majority of the members of the profession, we shall not attempt here their criticism. Our opinions upon the causes, spread, and communicableness of each of the three great epidemics just alluded to, have been previously expressed, and that expression is already in the hands of the publisher. Their repetition here would be altogether out of place, as well as an expenditure of valuable space, that can else be better appropriated.

In a work that is so uniformly well done, it is hardly proper to make any specifications of excellence. But, having referred to epidemics, we cannot resist the inclination to refer to the articles on the following subjects, as being of special interest, viz.: scrofula, tubercular consumption, scarlet fever, erysipelas, dysentery, pneumonia, dropsies, and fevers in general.

Poisons are individually considered in relation to their peculiar effects and special treatment. *Insanity* and *suicide* are other subjects which are thoroughly investigated in their causative and developmental phenomena, and a general view given of the manifestation of mind by the instrumentality of the brain.

The study of anatomy and physiology, of general and special pathology, of materia medica and pharmacy, has one grand culminating

*Author's Preface, p. 13.

object—the cure and prevention of disease, the mitigation of human suffering, and the prolongation of life. What matters it to know the causes of any disease, if we have not the knowledge or the power to bring to bear preventive or remedial appliances? Of what avail is it to suffering humanity, if we understand the morbid anatomy and the minute changes which the microscope only reveals in diseased structures, if we have not a corresponding knowledge of appropriate curative agents? Of what practical import is it that we understand the functions of the nerves, their cerebro-spinal connection, and influence over sensation and motion, if we cannot mitigate the racking pain of a neuralgia, or quiet the jactitations of eclampsia? All medical study, hence, should have a practical object; and that object should be, as above stated, the prevention and cure of disease, the mitigation of suffering, and the prolongation of human life. To know what ails the patient, and what is his minute pathological condition, avails the sufferer naught, and serves us no other purpose than as an indication of the source and means of cure, or at least of mitigation. It is here, in the department of special therapeutics, that Dr. Copland excels. We believe the volumes before us contain more reliable practical information than can be found elsewhere for twice the cost. "The author has endeavored to develop, under this head, the principles and intentions which should guide the student and the practitioner of medicine in the application of the knowledge they have acquired of other departments of science to the grand objects of curing and of alleviating diseases." —(*Preface.*) More than this, he has recorded the opinions and practical experiences of the lights in the profession, of the present and past ages, and overshadowing all are the results of his own extended experience and mature deliberations.

The diseases of women and children enter into the plan of the work before us, and these subjects are handled with signal ability, and with evident thorough acquaintance with all the recent publications upon those subjects. Dr. Copland was, for twenty-five years, Consulting Physician to a Lying-in Hospital, and, for many years, Physician to the Royal Infirmary for Diseases of Children, and is, consequently, thoroughly prepared to do justice to these specialties.

Each article is accompanied with copious bibliographical references, which will greatly facilitate the researches of the specialist, or any person who may desire to prosecute any subject of inquiry considered in the Dictionary of Dr. Copland. The work concludes with an index and classified contents, which, though condensed into the smallest possible compass, occupies over one hundred closely-printed pages.

It is proper here to refer to the editorial labors of Dr. Lee. This labor could not have been confided to abler hands, nor to one who would have discharged his duties with greater devotion or more signal ability. Ordinarily we have no great regard for editorial annotations, but in the present work they have been so ably made and judiciously timed as to greatly add to the value of the volumes, especially to American practitioners. The additions are very numerous and able, and the zeal of the editor is only surpassed by the industry of the author.

We have thus briefly endeavored to give the reader, who may unfortunately be unacquainted with Dr. Copland's writings, some idea of his great work—a work upon which he has industriously labored for thirty years, and the publication of which, in numbers, was commenced nearly twenty years ago, in England. The first volume was issued, by the present publishers, in this country, in 1855, and the last volume during the present year. Dr. Johnson once said, "It is the great excellence of a writer to put into his book as much as his book will hold." If this be so, then Dr. Copland is particularly excellent, for the last volume contains over 1,700 double-columned and fine-typed pages. Should another edition be called for, as we hope there may be, we would suggest to the publishers that they issue the work in larger type, and put it up in six instead of three volumes. Issued in the same type and sized pages as *Wood's Practice*, Dr. Copland's work would make eight volumes, of over 800 pages each. It is true, such an edition would increase the cost, but the work is worthy of such an issue, and should meet with a sale that should justify such an outlay.

We know that the whole profession will congratulate Dr. Copland upon the completion of a work that has constituted the labor of his life. A labor of such magnitude has never previously been attempted, nor, in its completion, displayed such profound research, such vigor, breadth, and comprehensiveness of intellect, as are evidenced in the work before us.

We cannot better conclude this notice than by a quotation from the *British and Foreign Medical Review*: "The information amassed in these volumes is literally enormous, and, contemplated simply as an accumulation, it must excite astonishment as the production of an individual; but when it is further considered that the whole of the materials have been most carefully selected from all existing sources, most particularly studied, valued, winnowed, digested, elaborated, and arranged into compact and simple forms, easily accessible and readily available in practice, it is not easy to point out, in the whole of medi-

cal literature, any work by a single hand so much calculated to excite admiration of the industry and talents of the author. In every article contained in the volumes, the reader cannot fail to be struck with the writer's most extensive learning, which has enabled him to collect knowledge from all authorities, ancient and modern, foreign and domestic; and he will, at the same time, be no less surprised than gratified at the singular power which has arranged the whole so lucidly and in such systematic order. As there is no medical practitioner in this country, old or young, high or low, who will not derive great pleasure and great profit by consulting them, so we think there is no one who should not add them to his library." The American edition contains voluminous and important additions, and is even more worthy of Dr. Forbes' commendation of the London edition, as given above. The American *editor and publishers* have done a good work, and should receive their reward. O. C. G.

A Treatise on the Venereal Disease. By JOHN HUNTER, F.R.S. With copious Additions, by Dr. PHILIP RICORD. Translated by FREEMAN J. BUMSTEAD, M.D., &c. Second edition, revised, containing a Résumé of Ricord's recent Lectures on Chancre. Philadelphia: Blanchard & Lea. 1859.

In all parts of the world the work of John Hunter on Venereal Diseases is as well known to medical men as the works of Shakspeare are to the lovers of dramatic poetry, and we do not deem it necessary to occupy any time in discussing the well-known merits of the author, whose name gives a title to the book under consideration. But around the work of John Hunter are ranged the notes of Ricord, Babington, Fournier, and, last of all, Dr. Bumstead.

M. Fournier is interne at the Hôpital du Midi, and has published a volume of the recent lectures delivered by Ricord on the subject of chancre. The translator has incorporated parts of Fournier's work into the present edition, and thus the changes which have taken place in the views of Ricord are scattered through different parts of the work. To our mind, it is a question whether the profession in this country would not have preferred a translation of the entire volume of M. Fournier, rather than to have had it eviscerated and served up in fragments, intermixed with portions of other authors. As Dr. Bumstead's book now appears, there is a lack of method in the arrangement and discussion of various important subjects, which produce no little confusion in the mind. The reader feels the need of exercising con-

stant vigilance, else he will be in doubt precisely whose opinions he is studying—whether those of Babington, Ricord, Fournier, or those of the translator; in short, there is a lack of that symmetry and clearness so desirable in all extended monographs.

There are many highly interesting matters ably discussed in this book, and whoever has leisure to read it through with care, and in the exercise of a sound, discriminating judgment, will be amply paid for the amount of time spent over its well-printed pages. Statements occasionally occur, however, which are at variance with our own experience, and we believe with that of most men in the profession. The limits of a short notice will not permit us to enter into an elaborate discussion of all these—one only must suffice, by way of illustration. The American editor asserts that there can be no infecting chancre without a corresponding indurating bubo. This, as a universal rule, is certainly incorrect; we have under treatment, at this time, a man who a few months ago had an infecting chancre, and now has constitutional syphilis. He has never experienced the slightest abnormal condition of the inguinal glands on either side. We could advance other instances from our own observation.

While we heartily commend the volume to favorable consideration, from the great amount of entertaining and scientific matter which it contains, we feel ourselves obliged to say that, as a methodical work for easy reference and consultation in the management of venereal diseases, it is not so well adapted to the wants of general practitioners as several other treatises lately published on the same subject. A.

SELECTIONS.

Spontaneous Generation.

It was as easy for the ancients to conceive that animals could be produced from putrefying matters, as it is difficult for the instructed physiologist of our day to conceive any generation whatever, except that by *direct parentage*. Aristotle found no difficulty in believing that worms and insects were generated by dead bodies, and that mice could become impregnated by licking salt. The successors of Aristotle were even less skeptical than he. They were constantly observing animals and plants suddenly springing into existence where no animals or plants had been before. Every dead dog or decaying tree was quickly beset with numerous forms of life; how could it be doubted

that the putrefaction, which was observed as an *invariable accompaniment*, was the *necessary cause* of these sudden appearances of life?

To the mind imperfectly acquainted with the results of modern science, spontaneous generation is as easy of belief as it was to Aristotle. Do we not constantly see vegetable mould covering our cheese, our jam, our ink, our bread? Do we not, even in air-tight vessels, see plants and microscopic animals develop where no plants and animals could be seen before, and where, as we think, it was impossible that their seeds should have penetrated? And when we hear that Mr. Crosse produced an insect by means of electricity, startled as we may be, do we really find any better argument than our prejudice for disbelieving such a statement? Where do parasitic animals come from, if not spontaneously generated in the body? These parasites are found in the blood, in the liver, in the brain, in the eye, nay, even in the excessively minute egg itself. "How *gat* they there?" is our natural question. This question, which is so easily answered on the supposition that generation can take place spontaneously, presents the most serious difficulties to science, because the massive weight of scientific evidence has been year after year accumulating against such a supposition; until the majority of physiologists have come to regard it as an axiom, that no generation whatever can occur, except by direct parentage. This axiom, which a small minority has always rejected, has quite recently met with a formidable questioner in M. Pouchet, the well-known physiologist of Rouen; and his experiments and arguments having agitated the Academy of Sciences, our readers may be interested if a review of the whole subject be laid before them.

The first person who assailed the notion of spontaneous generation was Redi, the excellent Italian naturalist, to whom we owe so many valuable observations. I have at this moment on my table the brief, but pregnant treatise, "Experimenta circa Generationem Insectorum," in which he reviewed the facts, and proved that the worms and insects which appear in decaying substances, are really developed from eggs deposited in those substances by the parents. So masterly was the treatise, that no one since then has had the courage to maintain the production of worms and insects spontaneously. It has been held as preposterous to suppose that putrefaction could generate an insect, as that it could generate a mouse—which Cardan believed. Driven from the insect world, the hypothesis has sought refuge in the world of animalcules and parasites; and there the hypothesis is not so easily defeated. Who ever turns over the pages of old Leeuwenhoek, the first who extensively applied himself to microscopic observations, will see that the Dutchman steadily set his face against spontaneous generation, because the microscope showed him that many even of these minute animals had their eggs, and were generated like the larger animals. Since that time thousands of observers have brought their contributions to the general stock, and each extension of our knowledge has had the effect of narrowing the ground on which the "spontaneous" hypothesis could possibly find footing; the modes of genera-

tion of plants and animals are becoming more and more clearly traced; and the necessity in each case of a parent-stock is becoming more and more absolute. It is true that there are organic beings of which, as yet, we can only say that there is the *strongest presumption* against their being exceptions to the otherwise universal rule of generation. We do not know, for example, how the *Amœba* arises; no one has ever seen its eggs; no one has ever seen its reproduction; and, what is more, it is perfectly easy *to make them* in any quantities. I have done so repeatedly. Nevertheless, they can only be "made" under the conditions which would be indispensable for their birth and development if they were really generated from eggs; and that they *are* so generated is a presumption which has every argument in its favor, except the direct evidence of the eggs themselves. The question, then, comes to this: Is it more probable that a law of generation which is found to reach over the whole organic world should have an exception, or that our researches have not yet been able to detect the evidence which would bring this seeming exception also under the law? One after the other, cases which seemed exceptions have turned out to be none at all; one after the other, the various obscurities have been cleared away, showing one law to be general; and it is therefore the dictate of philosophic caution, which suggests that so long as we remain in positive ignorance of the actual process, we must assume that in this case also the general law prevails.

Positive evidence would of course settle the dispute; but every one who has made any experiments, or has attentively followed the experiments of others, will admit that it is excessively difficult to devise any experiment which shall be conclusive. The facts elicited admit of such different interpretations; the avenues by which error may enter are so numerous. I will not narrate here the experiments of Fray, Gruithuisen, Burdach, Baer, and others, since they cannot withstand serious discussion; nor will I adduce my own, for the same reason. But those recently made by M. Pouchet have a more imposing character, and demand the strictest examination.

The reader will observe that the cardinal point in the investigation is to be certain that no organic germs could by any possibility be present in the liquid which is to produce the animalcules. On the hypothesis that the animalcules, like other animals and plants, are produced from germs, or eggs, these germs must be excessively minute, and easily overlooked.

If they exist, it is in the water and the air, awaiting the proper *conditions* for their development. Supposing them to be floating about in the air, under the form of dust-like particles, they would fall into, or enter, any vessel containing organic matter in a state of decomposition, and *there* develop; as the deposited eggs of the insect developed in the decaying body of the dog. Now, inasmuch as the presence of atmospheric air is one of the indispensable conditions of vitality, and without it the animalcules could not develop and live, the initial difficulty is how to secure the presence of this air, and yet be sure that the air itself does not *bring* with it the germs of the animalcules which

we find in the liquid. Schultze, of Berlin, devised an experiment which was thought to have finally settled this ponit, and to have refuted the hypothesis of Spontaneous Generation. An account of this experiment, to be found in the *Edinburgh Philosophical Journal* for October, 1837, shows that an infusion of organic substances, supplied with atmospheric air, but not with an air containing living germs, was suffered to remain thus, from the end of May till the beginning of August; but, during the whole of that time, no plant or animal was developed in the infusion. The apparatus was now removed from the flask, atmospheric air was allowed to enter freely —without first passing through the acid or potass—and in three days the infusion wass warming with animalcules.

This really looked like a conclusive experiment. No sooner were measures taken which would destroy the germs, supposed to be suspended in the atmosphere, than the infusion was kept free from animalcules; no sooner was the air allowed to enter the flask in the ordinary manner, than animalcules abounded. The proof did not, however, seem to be quite rigorous. It was by no means clear that the air in its passage through sulphuric acid would not suffer some alteration, perhaps electrical, affecting its vital properties; and this doubt seemed confirmed by the experiments of M. Morren, communicated to the French Academy, May 22, 1854; from which it appeared that air having passed through sulphuric acid was incompetent to sustain life, since the animalcules subject to it died in a few days. But M. Pouchet announces experiments which, if correct, not only scatter this doubt, and M. Morren's confirmation, but point-blank contradict the experiment of Schultze. He declares that in following Schultze's experiment in every particular, and also in repeating it with fresh precautions, he can constantly exhibit animalcules and plants developed in an infusion in which every organic germ has been previously destroyed, and to which the air has only access after passing through concentrated sulphuric acid, or through a labyrinth of porcelain fragments at red-heat. Nay, M. Pouchet goes further. Feeling the difficulty of satisfying his opponents, that the atmospheric air really contained no germs, he determined on substituting *artificial* air. This he did in conjunction with a chemist, M. Hougeau. Artificial air, as the reader knows, is simply a mixture of twenty-one parts of oxygen gas with seventy-nine parts of nitrogen gas. This air was introduced into a flask containing an infusion of hay, the hay having previously been subjected, for twenty minutes, to a heat of 100 degrees Centigrade, (212 degrees Fahrenheit,) a temperature which would destroy every germ. He thus guarded against the presence of any germs, or animalcules, in the infusion, or in the air. The whole was then hermetically sealed, so that no other air could gain access. In spite of these precautions, cryptogamic plants and animalcules appeared in the infusion. M. Pouchet repeated the experiment with pure oxygen gas instead of air, and with similar results.

In presence of such statements as these, only two courses were open to the antagonists of spontaneous generation. They could deny

or disprove the facts; or they could argue that the precautions taken were not sufficiently rigorous to exclude the presence of germs. I have already said how difficult it is for the modern physiologist to admit spontaneous generation, and the reader will be, therefore, prepared to hear that M. Pouchet has roused immense opposition; but the opponents have not disputed his facts; one and all, they accept the statements as he makes them; and by criticism, and counter-statement, endeavor to show that spontaneous generation is just as inadmissible as ever. These criticisms, and M. Pouchet's replies, may here be grouped in order, and with all possible brevity.

Milne-Edwards objected to the conclusions of M. Pouchet, saying: There is no proof that the hay itself had been subjected to the temperature of 100 degrees Centigrade, (or the boiling point of water,) it being very probable that although the furnace was at that heat, the hay, which was in a glass vessel and surrounded with air at rest, was not at anything like that temperature.

To this M. Pouchet replied, that he and M. Hougeau ascertained that the hay *was* at the temperature of 100 degrees, before they proceeded in their experiments.

Milne-Edwards is ready to grant that the temperature may have been reached, but argues that, even that would not suffice for the destruction of all the germs, if they were perfectly dry. He refers to the observations of M. Doyère, which prove that the *Tardigrada* ("water bears," microscopic animals, common in stagnant water,) when thoroughly desiccated, preserve their power of reviving, even after having been subjected to a temperature of 140 degrees Centigrade, (316 degrees Fahrenheit.) If, therefore, animals of so complex a structure as these water-spiders, can resist the action of so high a temperature, there is no reason for supposing that the germs of the simpler animalcules would be destroyed by it. Not content with this argument, which is sufficiently forcible, Milne-Edwards narrates an experiment of his own, which is very similar, both in method and results, to one I have performed. Unhappily, it is an experiment the value of which is either destroyed by the argument just adduced, or else it destroys the argument. It is this: In two tubes a little water containing organic matter is placed, one of them hermetically sealed, the other left open to the air. They are then placed in a bath of boiling water, and kept there till their temperature has reached that point. After this they are left undisturbed for a few days. In the tube which was exposed to the air there were animalcules; in the tube which was excluded from the air, before the action of heat had destroyed all the germs, not an animalcule could be seen.

Is not this something like a proof? "Why, no, sir," as Johnson would have said. At least, not if the argument previously urged is worth anything. Because every one will see that if it be true, as Milne-Edwards maintains, that the temperature of boiling water is not by any means high enough to destroy the organic germs of animalcules, then it could not have destroyed those germs in the closed tube, and animalcules ought to have made their appearance there. If I could

lay any particular stress on my own experiments, (which I do not,) they would lead to the conclusion that the organic germs do *not* resist the action of boiling water; for I found that a piece of fish divided into three, and placed in boiling water in three different tubes, one closed and excluded from the light, the second closed, but exposed to the light, and the third open, and exposed to the light, gave me *no* animalcules at all; had there been any germs in the water or meat, these must have been destroyed. But all such observations go for nothing in the presence of M. Pouchet's assertion that he had found animalcules in the infusion after subjecting the organic matters to a temperature of 250 degrees Cent., (546 degrees Fahr.,) and this, too, with *artificial water*. Unless the germs are supposed to be *incombustible*, it is difficult, he says, to maintain, after this, that the animalcules were developed from germs.

Milne-Edwards being thus disposed of by M. Pouchet, let us see how M. Quatrefages will come off. He says, that having examined the dust remaining on the filter after some observations on rain-water, he found that the organic elements presented a confused assemblage of particles; and this continued to be the case for a few minutes after their immersion in water. But a few hours afterwards, he detected a great number of vegetable spores, infusoria, and those minute spherical and ovoid bodies familiar to microscopists, which inevitably suggest the idea of eggs of extremely small dimensions. He also declares that he has frequently seen monads revive, and move about after a few hours of immersion. The conclusion drawn is, that the air transports myriads of dust-like particles, which have only to fall into the water to appear in their true form as animalcules.

The reply of M. Pouchet is crushing. If the air is filled with animalcules and their eggs, they will of course fall into *any* vessel of water, and as water is their natural element, will there exhibit their vitality. But if half a dozen vessels of distilled water, perfectly free from animalcules, be left exposed to the air, beside one vessel of distilled water containing organic substances in decay, the half-dozen will be free from animalcules and eggs, but the one will abound with them. Now, it is perfectly intelligible that, inasmuch as organic matter is said to form the indispensable condition for the development of the eggs, it is only in the vessel containing such matter that the eggs will develop; but why are they not also visible *as* eggs in the other vessels? why are not the animalcules themselves visible there, as they were in the water examined by M. Quatrefages? If both eggs and animalcules are blown about like dust in the air, it is an immense stretch of credulity to believe they will only be blown into the vessel containing organic matter; but the opponents of spontaneous generation go further even than this, for they declare these dust-like animalcules will be blown into a *closed* vessel, if it contain organic matter, but not into several *open* vessels, if they only contain distilled water.

M. Quatrefages is on better ground when he rejects the evidence, long supposed to be so weighty, of parasitic animals. He refers to the modern investigations which have not only made the generation of

these parasites intelligible, but in many cases have demonstrated it. M. Pouchet's reply is feeble, and unworthy of a physiologist of his eminence. He doubts the truth of the results obtained in Germany, Italy, and Belgium: "the monopoly of which," he adds, "has, by a strange anomaly, belonged to foreigners." Because France has not the honor of this splendid discovery, the Frenchman begs to doubt its value! Every physiologist, however—not French—will be ready to admit that whereas the parasitic animals formerly furnished the advocates of spontaneous generation with their most striking illustrations, the investigations of Von Siebold, Van Beneden, Küchenmeister, Philippi, and others, have entirely changed the whole aspect of the question, and given the opponents of spontaneous generation new grounds for believing that in time all obscurities will be cleared away, all contradictions explained.

In conclusion, I must say that, as far as regards the particular discussion, M. Pouchet seems to me to have the best of it. Their objections to his experiments are all set aside. If the facts are as he states them—and his antagonists at present do not dispute the facts—their criticisms go for very little. They have not shown it probable that any germs could have been present, under the conditions stated by him. Are we, then, to accept spontaneous generation as proven? By no means. It is very far from proven. The massive preponderance of fact and argument against such an hypothesis forces us to pause long before we accept it. What M. Pouchet has done is to destroy many of the arguments against spontaneous generation, and to have devised experiments which may finally lead to a conclusion. It is still on the cards that some source of error, as yet overlooked, vitiates his experiments; but until that error has been detected, he must be considered to have on his side the evidence of experiment, whereas we have on our side the massive evidence of extensive inductions. His experiment may be conclusive, and an exception to the general law will thereby be established. But it may also, on further investigation, turn out to be illusory; some little oversight may be detected which will rob the experiment of all its force.

Perhaps you will ask why this suspicion should be entertained? Why ought we not to accept M. Pouchet's statement with confidence, although it does contradict our inductions? The reason can only be, that the massive weight of these inductions naturally predisposes the mind to believe that it is more probable the experiment which contradicts them should be misconceived, than that they should be contradicted. Two years ago I became acquainted with an observation made by Cienkowski, the botanist, which seemed finally to settle this question of spontaneous generation, to place the fact beyond doubt, because it caught nature in the act, so to speak, of spontaneously generating. Cienkowski's statement is as follows: If a slice of raw potato be allowed to decompose in a little water, it will be found, after some days, that the starch-grains have a peculiar *border*, bearing a strong resemblance to a cell-membrane. This shortly turns out to be a real cell-membrane, and is gradually raised above the starch-grain,

which grain then occupies the position of a cell-nucleus. Thus, *out of a grain of starch, a cell has been formed under the observer's eye.* Inside this cell, little granular masses are developed, which begin to contract. Finally, minute eel-like animalcules are developed there, which bore their way through the cell-wall into the water.

Funke, in his report of this observation, which he says he has verified, asks, How is it possible to deny spontaneous generation here? Before our eyes a grain of starch becomes a cell, in that cell are developed living forms, which bore their way out.

The reader will imagine the sensation which such an observation created. He will agree with Funke, as I did, that if the fact were as he stated it, all discussion was at an end. But *was* the fact as stated? I tried in vain to verify it. Not less than twenty separate potatoes were employed, always in conjunction with ordinary starch, as a point of comparison; but although the animalcules were abundant enough, I never could satisfy myself of the first and all-important step, namely, the formation of a cell-wall round the starch-grain. This was the more distressing, because it is at all times unpleasant to be unable to verify an observation, especially one made by a careful and competent observer, and described in precise terms.

I could not reject what Cienkowski had positively affirmed, and Funke positively confirmed, and was willing to suppose that there was some necessary condition in the observation which I had not fulfilled. On the other hand, I could not reject a doctrine on the strength of a fact about which any doubt was permissible. In this state of suspense I had the satisfaction of hearing from Professor Naegeli, the celebrated microscopist, that he too had been baffled at first in the attempt to verify this observation, but that, after nearly a hundred trials, he had succeeded. He positively confirmed all the statements Cienkowski had made. But, from that moment, my suspense vanished. If the phenomenon was of such rare occurrence, there were reasons for suspecting some other explanation than that of spontaneous generation. What the source of the error was might not be easily divined; but it seemed very probable that error had crept in somewhere.

In the last number of the *Annales des Sciences Naturelles*, (x. 140,) there is a note which clears up the whole mystery. Cienkowski has himself discovered the source of his own error. The membrane which seemed to form itself round the starch-grain has had quite another origin. He has observed the little monads swimming about, and has noticed one of them *adhere to a starch-grain, spread its elastic body round it, and finally envelop it, as the Amœba wraps itself round its food.* This explains how the starch-grain comes to be inside a cell; and as this process was never suspected, and the starch-grain was seen with a cell-wall, the idea of natural formation was inevitable; the more so, as the wall seemed to grow larger and larger.

Thus has even this, the most striking case in favor of spontaneous generation ever adduced, been finally cleared up; and the reader will probably agree in the conclusion to which the whole of the facts advanced in this paper lead, namely, that the law of generation is uni-

versal; the exceptions which have been hitherto urged have, one by one, been found to be no exceptions; and the presumption is that even M. Pouchet's cases will be likewise explained. It is quite *possible* that the generation of animalcules may take place spontaneously; but although possible, it is not probable, and certainly is not proven.

GEORGE HENRY LEWES.

Diphtheria.

Dr. David Wooster thus sums up an able article on Diphtheria, which has appeared in the *Pacific Medical and Surgical Journal:*

I. Diphtheria is a *specific* disease.

II. It is distinguished from scarlatina, by the absence of eruption; from gangrenous sore throat, by the absence of ulceration and sloughing; from croup, by the aplastic nature of the exudation.

III. Diphtheria may properly be divided into two varieties: the mild and the severe.

IV. The mild is seldom fatal: slight, or no difficulty of deglutition, little fever, no engorgement of cervical glands, neither coryza nor lachrymation, but presenting the positive diagnostic sign of aplastic exudation on the tonsil, palate, or pharynx.

V. The severe is recognized by the diagnostic aplastic false membrane, high fever at first, coryza, lachrymation, engorged glands about the jaw, difficult deglutition, difficult utterance, or complete aphonia, great diminution of animal power, cyanosis, vomiting towards the close of the affection, and intense gangrenous fœtor from the decomposition of the exudation.

VI. Diphtheria is contagious.*

VII. The simple form is easily controlled by treatment which is the same as for the severe form, modified according to the comparative urgency of the case.

* The experience of the French epidemics has made abundantly clear one very important fact in the history of diphtheria, which has not yet been so clearly eliminated from the observed facts of the English epidemic. It may be very clearly shown, by the evidence collected, that contagion plays the principal part in the propagation of diphtheria. There is in this country a great deal of skepticism as to the contagious character of this disease; but the mass of evidence to prove it is overwhelming. Thus, Bretonneau has collected some crucial cases. One is that of M. Herpin, surgeon to the hospital at Tours, and professor at the school. A child seized with diphtheria, who had transmitted the disease already to its nurse, was placed under his care: and at one of his visits, by access of cough, part of the diphtheritic matter was ejected from the mouth while the process of sponging the pharynx was being performed, and it lodged on the aperture of the nostril of M. Herpin. Occupied with his task, he neglected for a moment to remove it. A severe diphtheritic inflammation of the part ensued, which spread over the whole nostril and pharynx. Extreme constitutional disturbance occurred, and the prostration was so severe that convalescence occupied more than six months. Dr. Gendron, of Chateau de Loire, received a shower of tracheal diphtheric exudations expelled by a young patient during an access of coughing. Laryngeal diphtheria set in, with urgent symptoms. Prompt measures saved him.—*Lancet*, April, 1859, p. 288.

VIII. The severe form is with difficulty controlled, and the diagnosis is always extremely unfavorable, even at the beginning.

IX. The treatment is the local application of hydrochloric acid, diluted or not; solution of nitrate of silver, one or two scruples to the ounce of distilled water; strong solution of sulphate of copper, drachm and a half to the ounce of water; concentrated solution of the perchloride of iron; Monsel's Salt, in powder; solution of chloride of sodium, etc., according to the educated judgment of the physician.

Externally—strict cleanliness; in the first stage of the disease, while the engorgement is red and hot, cold, wet compresses applied to the neck, and often repeated, can scarcely fail to relieve, at least for a time. Farther along, when the engorgement of the throat becomes œdematous, warm fomentations should be substituted. Never liniments, mustard, nor anything of the kind; for these appliances increase the anguish of the patient, and do not mitigate the affection.

Internally—first, a thorough emetic of ipecacuanha, given in full dose and largely diluted, so as to be vomited immediately. This should be repeated daily for the first two or three days. The bowels should be moved once a day by glysters, if possible; if not, by some mild laxative. Drastic cathartics should be carefully avoided. Iced milk and water, or iced gum-water, or iced infusion of ulmus fulva, should be given in minute quantity, frequently, as the patients desire. These iced fluids will be found very grateful to them.

Quinine in small doses, say from one-fifth to one-half a grain, often repeated, should materially assist in the tonic treatment universally recommended.* The mixture of "sesquichloride of iron with chlorate of potass, chloric ether, and hydrochloric acid sweetened with syrup," may be employed by those who fancy many not incompatible compounds in one conglomerate. But care must be used not to give this mixture with the milk and eggs recommended in the same paragraph of the *Lancet's* Commission; and yet it is hard to see how this could be avoided, if all are to be given "frequently." I would suggest another remedy combining tonic, nutriment, and stimulus: ferruginated cod-liver oil and brandy, in the proportion of two fluid drachms of the former to one of the latter, repeated four or five times a day, or oftener, if the stomach will bear it.

I believe chlorate of potash harmless, but of unproved efficacy in diphtheria. I have employed it, but saw no results except mitigation of fœtor of expiration, when the false membrane was in the stage of

* Of the many internal remedies which have been advised, we do not know of any on which so much reliance can be placed as on the tincture of sesquichloride of iron, with chlorate of potass, chloric ether, and hydrochloric acid, in the form of mixture, sweetened with syrup, full doses being employed, according to the age of the patient, and frequently repeated. A free use should be made of generous wine, beef-tea, coffee, eggs in combination with brandy and wine, milk, and whatever other form of nutriment the ingenuity of the surgeon or the fancy of the patient can suggest. When food is refused, then enemata similarly composed must be administered frequently, in small quantities of two ounces and upward, that they may not be rejected; for it is of the first importance that inanition should not open the last portals of life to the advancing disease.—*Lancet Commission.*

decomposition. Charcoal, chloride of soda, etc., perhaps act in a similar manner. Ablation of the tonsils at the beginning of the disease, it appears to me, cannot be too strongly insisted upon. Tracheotomy cannot be ignored, but should be resorted to where other means fail to relieve the cyanosis, and suffocation is imminent from obstruction of the fauces and larynx, or even the fauces alone, the larynx being intact. In this latter condition, tubing the larynx should first be attempted.

It is less difficult to prevent this disease than to cure it. The most scrupulous cleanliness of person and surroundings, free and constant ventilation, should be insisted on. If there are many children, and the rooms are small, as frequently happens in families where this disease appears, the well ones should be sent away, if possible; and if not, kept out of the room in which the infected child lies. M. Duche insists on the free use of sulphur as a prophylactic: he says those children who took sulphur were not attacked, in any case; while others, under the same circumstances, fell victims to the malady. It is a simple, harmless remedy, and should be tried as a preventive. M. Duche may be right.

Finally, all our knowledge of this disease may be thus epitomized:

1. Diphtheria is a specific, new, zymotic disease.
2. Its diagnostic sign is an aplastic membranous exudation.
3. The indications of treatment are to remove the exudation, and prevent its re-formation.
4. The treatment is tonic, antiseptic, stimulant, and nutritious.
5. The means of prevention are cleanliness, pure air, free living, and possibly sulphur taken internally.

CONCLUSION.—Diphtheria is the most *certainly* fatal epidemic that ever visited our race; but it is not *de natura sua* incurable.

Atropine in Epilepsy.

Dr. Max. Maresch, availing himself of his position as physician of an establishment for the insane, at Vienna, has submitted some epileptic patients to the influence of atropine, and has published the results in the *Vienna Journal of Medicine*. Dr. Maresch's experiments were made upon eight patients in the female department, and ten more in the department of the incurable insane; four of these were men, and six women.

Of the eight first patients, three were completely cured, and the condition of the five others was so notably ameliorated, that it was impossible to deny the beneficial effect of the atropine. Of the ten patients belonging to the class of incurables, eight experienced a marked diminution in the violence and frequency of their epileptic attacks, as well as in the exacerbations of their physical troubles. These results, united to those obtained by others in the treatment of epilepsy by atropine, merit serious attention.

Maresch has carefully noted the therapeutic phenomena which have

arisen during the administration of this remedy. One-fiftieth of a grain of atropine gave rise, in every case, to effects which habitually follow the administration of this agent, such as dryness of the throat, difficulty in articulating, visual aberration, dilatation of the pupils, etc.; phenomena to which by degrees the patients become habituated, which, however, do not cease during the entire treatment. In every case the pulse lost eight or twelve pulsations during the first hour after taking the remedy, but the pulse resumed its normal frequency so soon as the other therapeutic phenomena manifested themselves. There did not occur, in any case, a marked and permanent acceleration of the pulse under the influence of the dose above indicated. As special phenomena, Maresch observed in those cases while under the influence of atropine, an exanthem analogous to roseola, which soon disappeared under the influence of warm baths and the discontinuance of the remedy. It is, besides, worthy of remark, that atropine did not give rise, in any case, to digestive derangement, or any other unfavorable symptom.

The administration of this remedy has not proved beneficial in the other forms of mental diseases. Dr. Maresch administers atropine as follows: he dissolves a grain of it in five hundred drops of rectified alcohol, and of this solution he gives from five to ten drops, (from one-hundredth to one-thirtieth of a grain.) This dose is administered once, in the morning before breakfast, from which coffee, tea, and chocolate must be excluded, as these substances interfere with its action. This is continued from sixty to ninety days, without interruption, then is to be resumed after an interval of from thirty to forty-five days. With women, there is no need to suspend its administration during menstruation, as its favors and augments this discharge. Rarely does atropine give rise to constipation, more frequently to diarrhœa, which necessitates, when it becomes severe, a suspension of its administration for some days.—*Translated from L'Union Médicale, by* S. E. C. —*N. O. Med. and Surg. Journ.*

Reflections upon the Use of Raw Meat in the Colliquative Diarrhœa of Infants at the Breast. By Dr. J. F. Weisse, Director of the Hospital for Children, at St. Petersburg.

A period of seventeen years has elapsed since the attention of my confrères was called to the great value of this remedy in this disease, but it did not come into general use until five years later, and after the publication of more extensive works upon the subject. About this time, Dr. Behrend, of Berlin, addressed me a letter which contained the following passage: "You cannot imagine how much interest your communication upon the treatment of the colliquative diarrhœa of infants at the breast, by raw meat, has excited; we now use it exclusively in this disease." Soon after, Dr. Behrend inserted in the sixth volume of his journal, a letter from M. Marotte, Physician of the Central Office of the Hospitals of Paris, addressed to Professor Trousseau, in which the author gave a theory to account for the re-

sults which I had obtained. From this latter time, the treatment by raw meat has been generally admitted everywhere, and its utility has become incontestable.

Among the numerous favorable reports recently published, I will cite that of Dr. Eichelberg: "In consequence of the short time which has passed since this treatment was recommended, I have by me only a limited number of observations, (twenty,) but all prove its efficiency. The cases in which infants refuse raw meat are very rare; the majority swallow it with avidity. I have observed two cases which were very striking; in these the children took the meat for several weeks without repugnance; a very great amelioration was felt in the general system; all at once they refused it, as if guided by a natural instinct. The want of osmazome induced the infants to devour the meat, but as soon as the equilibrium was established in the economy, this want disappeared."

Dr. Eichelberg makes use of raw meat only in the treatment of diarrhœa which attacks children fifteen days or three weeks after they have begun to take the breast; the cure is then certain; for myself, in ordering this regimen, I also have employed it in this affection only; and now, after twenty years of experience, I maintain that raw beef, reduced to a pap by scraping, is, to the exclusion of all other medication, the veritable specific for this diarrhœa which causes so much ravage. I am not able to admit the assertion of Mr. Charles Hogg, in recommending beef-tea, so well known in England: "Beef-tea is an excellent aliment, very nutritious, and easy to digest; it replaces completely the juice of the meat obtained by scraping, which is so lauded by Dr. Weisse, of St. Petersburg." I have found raw meat not an aliment for infants, but a remedy for diarrhœa; further, I have never spoken of the juice of meat, but have recommended the muscular substance itself, hashed or grated in such a manner as to be easily swallowed and digested. The end proposed is to cause the muscular substance itself to pass into the digestive tube, and beef-tea has no more effect upon the diarrhœa of children at the breast, than the excellent decoction of meat, praised by Liebig. These two liquid aliments, from the simple fact of their fluidity, traverse too rapidly the intestinal canal. By giving the meat in pap, the solid parts sojourn longer in the intestine, acting by contact, and being able, by exciting the intestinal mucous membrane, to stimulate absorption; it is also probable that this means contributes to neutralize the acidity of the gastric juice. I cannot partake of the hope expressed by Dr. Beer, of seeing cod-liver oil replaced in the Materia Medica by hashed raw meat. Each of these excellent remedies has its sphere of action marked out in the treatment of the diseases of infants; raw meat combats the diarrhœa of the newborn; cod-liver oil triumphs over rachitic affections with or without atrophy.

The treatment of infants by raw meat has come into general use at St. Petersburg, and this use has been propagated rather by proofs of its good effects, than by special writings. The most part of our confrères, employing it when the ordinary means have failed, have also verified the good results obtained by myself. So far as it con-

cerns myself, I have employed this treatment in nearly two hundred cases, and the result has been always satisfactory when the disease was taken in time. I say when the disease was taken in time; for, if the malady is too far advanced, and has assumed the characteristics of gastro-malacia, (softening of the stomach,) a cure is rarely obtained. Sometimes, even in these circumstances, we may succeed in calming some symptoms so fatiguing to the little patient, such as the inextinguishable thirst, and the vomiting.

In a large number of patients cured by raw meat, verminous affections have been observed, and particularly tænia, otherwise very rare at St. Petersburg. Dr. Braun stated this as his opinion, and two years after, a high authority, Prof. Von Siebold, of Munich, expressed the same idea, in the last paper of his interesting work, "*Weber die Band und Blasenwürmer*," Leipsic, 1854: "We should no longer be surprised at the fact, and must credit those physicians who have declared that they have found in many patients submitted to a diet of raw meat, the tænia. And Herr Von Siebold has remarked that in every case the tænia solium was found, and he believes that in all probability, this worm, which is not indigenous to St. Petersburg, has been brought there by the cattle driven from Tocherkask and Podolia. Some weeks before my departure from St. Petersburg, in the month of June of the present year, a tænia, more than ten feet in length, was sent to me by a confrère, to whom I had warmly recommended meat, in the case of an infant aged 18 months. The tænia was expelled after the administration of the ethereal oil of male fern. This remedy was employed because the child, having been cured of its diarrhœa by the raw meat, had several times passed parts of tænia.

I ought not to forget to say that in the hospital for children, which is in my charge, I have often tried, but unsuccessfully, the raw meat in the diarrhœa of children more aged, who were sick from no cause connected with dentition. More frequently this diarrhœa was caused by ulcerations of the intestinal canal. [Excerpta translated from *L'Union Médicale*, by S. E. C.]—*N. O. Med. and Surg. Journ.*

Syrup of Coffee for Whooping-Cough.—When whooping-cough has resisted the agents most ordinarily used, the following syrup, which is the formula given by M. Delahaye, slightly modified, will be used with full success. We have experimented very often, says Dr. Courbassier, in the localities where whooping-cough appears each year with an epidemic character, and it has rarely failed us. Here is the mode of its preparation:

Take eight ounces of Mocha or Martinique coffee, slightly browned, in powder; treat by displacement with boiling water, so as to obtain sixteen ounces of infusion.

Dissolve in this liquid, alcoholic extract of belladonna, alcoholic extract of ipecac, of each ℥1¼; alcoholic extract of cinchona, gr. xxxvj; add sugar, ℥xvj. Digest on a water bath, and filter.

The dose for children of three or four years is a table-spoonful repeated three times a day. Under this age, the dose should be reduced one half.—*Revue de Thérapeutique*, from *The Druggist*.

The Cholera in Germany.—A letter from the Grand Duchy of Mecklenburg, in the *Augsburg Gazette*, says: "The cholera has broken out with great violence in some towns and villages of this duchy, and many of the persons attacked have died after only four or five hours' suffering. In some of the villages the harvest operations have been suspended for want of hands, sixty to seventy persons having been taken ill at the same time. The disease breaks out first in one place and then in another, sparing for a time intermediate villages, and then turning back on them with increased violence. The ports of Rostock and Warnemunde have not escaped the malady, which was brought there, it is supposed, by a vessel from St. Petersburg. The cholera continues to rage at Hamburg, carrying off from sixty to seventy persons daily."

Test for Sugar in Urine.—*L'Union Médicale* has given its readers long scientific details respecting the best method which the practitioner can use for the purpose of ascertaining the presence of sugar in the urine of diabetic patients; and this conclusion is satisfactory, as it shows that the method in ordinary use is the best of all methods: "It is evident," the report says, in conclusion, "that for the ordinary use of practitioners, the test by the aid of caustic potash, or by lime, is sufficient. A glass tube, a spirit-lamp, caustic potash, or a bottle containing milk of lime, (*lait de chaux*,) are all the articles necessary for the purpose."

M. LONGET has been nominated Professor of Physiology of the Faculté de Médecine of Paris.

EDITORIAL AND MISCELLANEOUS.

Voltaic Narcotism.—Dr. Benjamin W. Richardson, of the Royal Infirmary for Diseases of the Chest, publishes a paper with this title, in the *Medical Times and Gazette* for June 25, the substance of which will doubtless interest our readers. It is proposed to effect local anæsthesia by means of the electric current in combination with narcotic applications. He considers his process as producing deeper insensibility than the freezing process, with no induration of tissue, though the freezing process is, "for the present, most practicable." In comparison with chloroformization, it has the advantage of absolute safety as regards life. We are not prepared to give an opinion on the subject as yet, and only give the process as described by the author.

A narcotic solution is made of 15 grains of aqueous extract of aconite, dissolved in three drachms of spirit and four drachms of

chloroform; this is covered by a piece of clean copper, and connected with the positive pole, and a plate connected with the negative pole is so placed that the current shall pass through the part intended to be narcotized. The solution is applied by means of a small sponge. The battery employed was the chair battery—we presume that known in this country as Pulvermacher's.

In cutting operations on the human subject, the success seems to have been but partial. In a case of whitlow, the current was kept up for two hours, without producing any result except the most superficial insensibility. In a case of strangulated hernia, the following account is given of its effects:

"In preparing the patient for this operation, I placed a copper disc, with sponge beneath, charged with the narcotic solution of aconite and chloroform, and connected with the positive pole of the chain battery, over the line of the incision, while a copper plate, of horse-shoe shape, surrounded with moistened lint, and connected with the negative pole of the battery, was made to encircle the inner plate. The voltaic current was then set up. Owing to the circumstance that the arrangements had to be hastily put together, the circuit was several times broken, giving rise to sharp shocks, of which the patient complained very much. After half an hour the plates were removed, and the operation was performed. During the operation, the patient did not wince or move, but he gave indications as of passive suffering. This was most marked at the moment of the division of the stricture. The patient, afterwards, could give me no defined idea of what he had felt during the operation; and Mr. Hancock agreed with me that the severity of the operation had been effectualy broken. Mr. Wotten has since informed me, that for a few days after the operation there was some superficial vesication over the part where the narcotic solution had been applied, which caused a little inconvenience. The wound, nevertheless, healed favorably."

It will be observed that this case was of doubtful success; and the same remark may be made of others reported. In the removal of a tumor from the shoulder of a woman, after an hour's application of the current, the operation was performed by Dr. Pettigrew. "The patient was unconscious of the entrance of the knife, and of its course; but as the tumor was being dissected out, she gave expression as if suffering from some undefinable sensation, such expression as patients often give vent to during partial recovery from chloroform in the course of an operation. The tumor removed, and the patient calm, she expressed to us that she had no knowledge of having been cut, nor of

the passage of the needles on inserting the sutures; but at some point of the operation she experienced what she called a 'wrench,' as if something was being removed by force."

The result of the author's experience shows that this process *may* be useful in *some* cases, as an aid to the surgeon. The anæsthesia may be explained thus: the narcotics themselves are acting by absorption, and the continuous electric current simply quickens "the capillary circulation in the structures through which it is transmitted, and the absorption process is therefore more rapid and determinate."

Dr. Richardson mentions the objections to the process as these: the time required, the production of some slight pain, and a vesication which he thinks is produced by the chloroform. May not the latter be explained, as Dr. Bird suggested in 1847, by the fact that there is decomposition of chloride of sodium, the chlorine of which, being eliminated at the positive pole, unites with the copper, and forms a caustic chloride of copper? L. H. S.

— Who is not interested in the deaf and dumb? If any one can, without the deepest sympathy, witness their too often unavailing efforts to make themselves understood by their fellows, he must be destitute of a human heart. Public institutions have sprung up abundantly for the education of those who have arrived at the age of ten or twelve years, and this is probably as early as they can be well cared for in such establishments. Before this they need a more careful, more parental oversight than is possible, except in private institutions of a limited number of pupils. From the circumstances of the case, however, the number of persons who are qualified and inclined to take such a charge upon themselves is small, and it is not always easy for those who have charge of young mutes to know what to do with them, although they feel themselves to be entirely unfit to educate them. We believe, therefore, that we are doing a favor to our readers by calling their attention to a private school, in which mutes are received as early as six years of age, and which is under the charge of persons eminently qualified for such an enterprise. Mr. D. E. Bartlett, of Poughkeepsie, N. Y., a gentleman of liberal education, and of large experience in teaching deaf mutes, is the head of the school. Mrs. Bartlett and a sufficient number of teachers assist in making it *home-like* to their pupils, as well as in giving them instruction. And certainly we never saw a school of young children where all seemed to be so happy, at the same time that they were effectually taught the dry rudiments of knowledge. One feature of the school is especially worthy of notice: to wit, that pupils who can talk are also received. The

influence upon the mutes is peculiarly happy. They lose the shyness so often manifested by those who have been shut up to the society of their fellow-sufferers, while they also learn many things more rapidly from these companions than they would from their teachers. But the greatest benefit is derived by those mutes who can be accompanied by their brothers or sisters, for the speaking pupils are also taught the sign-language, and when the children return to their home the mute is not again shut out from direct communication with his family, as must be the case always when no one has been systematically taught to converse with him. Any one who may know of families in which there are mute children, would be doing a kindness to them, by informing them of the advantages offered in the school of Mr. Bartlett.

— A new agent of disinfection and for the dressing of gangrenous wounds furnishing an abundant and fœtid suppuration was recently presented to both the Academy of Sciences and the Academy of Medicine of Paris, by M. Velpeau.

The authors of this discovery are Dr. Demeaux and Mr. Corne; the one a medical practitioner in the South of France, and the other a veterinary surgeon. In their communication, presented by M. Velpeau, they make the following statements, which have been fully corroborated by experiments entered upon in the surgical wards of the distinguished professor, under whose auspices the report was made to the Academy:

1. A gangrenous sore, characterized by an abundant and fœtid suppuration, subjected to this mode of dressing, is almost instantly deprived of all disagreeable odor.

2. Twenty-four, and even thirty-six hours afterwards, the dressings of a fracture complicated with a wound of a bad character, emits no more odor than the dressings of a simple fracture.

3. An ulcerated cancer, giving off an ichorous discharge, with that fœtid odor which is peculiar to it, subjected to this manner of dressing, is almost instantly deprived of the odor, and remains so as long as the dressings are applied.

4. Ulcers of the legs are also freed from odor.

5. Apparatus used in dressing wounds, linen saturated with fœtid pus, poultices impregnated with suppuration, brought in contact with the disinfecting substance, immediately lose all disagreeable odor.

6. Infected fluids, products of gangrene, decomposed clots of blood, sphacelated tissues, the remains of bodies in a state of advanced putrefaction, treated in this manner, are instantly disinfected.

The action of this disinfecting substance seems to arrest the pro-

cess of decomposition; it keeps insects from the sore, and prevents the production of worms.

The disinfecting substance is composed of 100 parts of plaster of Paris and one to three parts of coal-tar. These two substances are rubbed together in a mortar, or in some other appropriate way, producing a powder of a grayish color, and of a slightly bituminous odor. It may be applied to the surface of sores simply in the shape of powder, or, by combining a certain quantity of the powder with olive oil, a paste or ointment is made, which will retain its properties for an indefinite time, if kept in a jar. This ointment is of a dark-brown color, and has a bituminous odor. The oil serves to make the powder cohere, without dissolving it, in such a manner that the new product, by the gradual elimination of the oil, does not lose the property of absorbing pus as long as it is placed upon a suppurating sore. Whether used in the form of powder or as an ointment, the consistence which it acquires is never such as to incommode the patient in the least, or to affect the wound unpleasantly. It can be applied either directly, or with a layer of some thin texture interposed between it and the sore, according to the case or the object to be attained. The direct application produces no pain; it has, in fact, a detersive action—an effect favorable to cicatrization.

This preparation has been used in both the Charity Hospital, by M. Velpeau, in dressing sores, and at the Veterinary School at Alfort, in disinfecting putrid matters, by M. Renault, its director. The latter, at the Academy of Sciences, related his experiments with different substances possessing the same characteristics as coal-tar, such as pitch, tar, creosote, &c., and considered that the pitch, with the plaster, produced equally good results, and had the advantage of not being of so unpleasant an odor as the coal-tar. These facts, M. Velpeau replied, were known to the discoverers of this preparation, but they found in the cheapness of the coal-tar an argument in favor of its general adoption.

The discussion on the subject occupied a portion of two sittings of the Academy of Sciences. At the last of these sittings, M. Velpeau remarked, that its continued use had been followed by results more and more satisfactory, and that it was incontestably a good preparation.—D.

— M. Duchense Duparc believes that he has found in Fucus Vesicularius a remedy for obesity, when it constitutes a disease. He has made several trials of it, and always with good results. He employs the whole plant, stem and leaves, either in decoction or in powder, in the form of pills.

— An article appeared in the *New York Journal of Medicine* during the early part of the year, having for its title "Why do our young men go to Paris?" In this article the author complained of the great mortality in the Paris hospitals in comparison with that in our own institutions, and spoke of the men whose names are familiar as those of the leading men in the profession of that city, as being heads of cliques, and devoured by professional jealousy. To these charges a writer—Dr. Giraud-Teulon—replies in the *Gazette Médicale de Paris*, denying the correctness of the statistics, and flinging back the charge of jealousy in comments upon the Whitney case, which he reviews in the following sentences:

"Professional jealousy, that deplorable wound of the medical body, is not an attribute particularly French, although we are forced to admit its disastrous influence upon public opinion in our midst. At the same time that the *New York Journal of Medicine* draws a not altogether flattering picture of our professional rivalries, we find in the AMERICAN MEDICAL MONTHLY a report of a long debate before the Academy of Medicine of New York, which is, in fact, a continuation of the discussion held here upon the tubage of the larynx—a continuation remarkable for its bitterness, as well as for its subject. At the same time that M. Bouchut replied to some misstatements in relation to his laryngean tube, one of his most celebrated predecessors in this question of tubage, Dr. Green, of New York, was accused of having caused the death of one of his patients by the unfortunate introduction of a laryngeal tube, and the injection—not less fatal—of a solution of nitrate of silver into the bronchiæ." A *résumé* of the case is then given, after which the writer proceeds to say:

"It is not necessary to state that Dr. Green had little trouble in defending himself. Could death have been caused by laceration of the tracheal mucous membrane? Assuredly not, as it was found perfectly healthy twelve days after the pretended fatal introduction of the instrument. Could it have been produced by the injection of the nitrate solution? This, too, is difficult to suppose, the patient having passed eight days comparatively well, and even better than before, going into the country, trying new horses, &c.; all this in the interim between the period when the injection was performed and the commencement of the symptoms of suffocation, which took place suddenly eight days afterwards. Everything leads us to recognize in this case a tuberculous cavity, perforating the pleuræ, and some tuberculous ganglions breaking down, at the same time, along and outside of the larynx. The emphysema is not difficult to understand, in connection with the

pneumo-thorax. Another thing, which is also not difficult to understand, is the culpability of the physicians, who placed their error of diagnosis to the account of an operation of an absent confrère. Thus seemed to think the Academy of Medicine, and we are not surprised at this sentiment.

"American physicians can then, with justice, stop their students from going to Paris to learn lessons of jealousy and rivalry. They, as we, have their professors." D.

— M. Eissen has recommended in the *Gazette Médicale de Strasbourg*, the use of quinine, introduced into the air-passages, for the treatment of intermittent fevers. The process consists in the inhalation of quinic ether, a combination made by M. Manette, and first used by M. Pignacci, of Milan.

The substance, still incompletely defined, in a chemical point of view, is obtained by means of the distillation of alcohol, treated by sulphuric acid, (theory of ethers,) in presence of the quinate of lime. The product of this distillation is a liquid perfectly limpid, colorless, of an agreeable odor, less volatile than sulphuric ether, but still sufficiently volatile to evaporate at an ordinary temperature without leaving a deposit. It deserves, then, the name of ether; and its therapeutical action, besides, seems to justify the qualification of quinic, which has been given to it.

Quinic ether, says M. Eissen, fills all the necessary conditions of a good therapeutical agent. It acts, at the same time, *tuto, cito et jucunde*. Inhaled in the dose of a few grammes, (2 to 3,) as chloroform is generally employed upon a compress, it checks a commencing access, and prevents the return of subsequent accesses. In all the cases in which it was tried, the access gradually yielded, never to return, when the fever was simple, or under a very mild form in cases of decided cachexia.

Since the first trial upon patients in Lombardy, who could not be suspected of being slightly affected, other trials have been made by Prof. Groh, at Olmutz, and with the same success. The results have been the same: whether the inhalation was made before or during the pyrexia, the access was lessened in character in a marked degree, and the next anticipated access prevented, in the majority of cases; the tumefaction of the spleen disappearing at the same time. The inhalation, far from being disagreeable, was followed by good results, or of a sensation of decided amelioration. We may add that, in their experiments, our learned confrères, whose names we have cited, were careful to establish negative proofs to confirm their judgment. They submitted a certain number of fever-patients to inhalations of pure

sulphuric ether, or sulphuric ether holding sulphate of quinine in solution. The inhalations of pure ether produced no other effect than that of increasing, in an insupportable manner, the hot stage; while in those taking the ether containing the quinine, some anti-periodic effects were observed, after large quantities of the remedy were absorbed; but in severe cases these effects were so slight, after long trials, that the patients themselves solicited more energetic measures.—D.

— It is well known that the infusion of coffee, tannin, camphor, vinegar, acetate of lead, fatty oils, white off eggs, &c., have been proposed in succession, as antidotes to plants, or their alcaloids, belonging to the family of the papaveraceæ, and the solanaceæ. Now, Dr. Garrod prescribes animal charcoal, which, in small quantities, neutralizes, or entirely destroys, the action upon the economy of the solutions of belladonna, of stramonium, and of hyoscyamus, provided that the antidote is administered before they are absorbed. Dr. Garrod cites, in the *Bulletin de Thérapeutique* for 1858, two cases where patients had taken, by mistake, one 60 centigrammes, the other 10 grammes of belladonna, and were immediately cured by the use of animal charcoal. Again, he administered to a dog a dose of aconite, which killed him promptly; while to another dog of the same size, which had taken four times as much aconite, but mixed with animal charcoal, presented no symptom of poisoning. M. Garrod has remarked that animal charcoal, purified or not, acts in the same manner, and that vegetable charcoal, on the contrary, has no effect whatever upon the action of these poisons. He also believes that animal charcoal neutralizes the stupefying action of all the vegetable alcaloids, as quinine, strychnine, and morphine. D.

Inductive Electricity, or Faradization, in Lead Colic.—Of late, much attention has been paid in France to the employment of electricity by induction, or Faradization, as it is called, in the treatment of certain diseases. Briquet, of La Charité, has employed this with success in the treatment of the paroxysms of colica pictonum. The application is not devoid of pain, and the latter is, at times, so great as to demand the anæsthetic aid of chloroform during the operation. A wet sponge is attached to one wire of the apparatus and placed at the upper portion of the recti muscles, and a metallic brush being applied to the other, it is passed over the part of the abdomen most painful, and this is continued until redness is produced, when the pain will disappear. The operation may require repetition in some cases; but if twenty-four hours pass without the reappearance of any pains, no further application of the process will be required. The after-treatment con-

sists in a sulphur bath every other day; the internal administration, daily, of about 4 pints of lemonade, containing from 30 to 40 drops of sulphuric acid.

"A gum potion is also prescribed, containing from 30 to 40 grains of alum; and each night, or every alternate night, as the case may be, a pill is ordered, containing one grain of the watery extract of opium. * * In almost all cases, Briquet has remarked that the bowels begin to act spontaneously from one to three days after the Faradization has accomplished the removal of the abdominal pains." This treatment is interesting from its simplicity as compared with the complex routine heretofore used at this hospital, in the treatment of colica pictonum. The latter extended over six days, and was probably one of the last relics of ancient medication to be found in modern hospitals. Our readers will find it given in detail in Nevins's translation of Trousseau and Reveil's Prescriber's Hand-Book. L. H. S.

Nocturnal Incontinence of Urine.—A writer in the *Bulletin de Thérapeutique* recommends the employment of mastic in these cases. It is given in pills made with syrup. It is necessary that 32 grammes should be administered in four days, if the child is over ten years of age; when under that age, the amount mentioned should be given in six or eight days. If a cure does not result from the first trial, a second trial with a like quantity should be made. But if the incontinence persists after the second trial, it is useless to continue the medication. The failures are, however, exceptional, as two-thirds of the cases have been successful, even in patients from 18 to 24 years, who had been affected with this disgusting infirmity from infancy.—L. H. S.

Guano as an Anti-Syphilitic!—WHAT NEXT!—It seems that guano had been proposed by some ardent searcher after novelties as a specific for all venereal affections, for what reason no one could imagine. Dr. Gamberini, of the Hospital of St. Ursula, at Bologna, has satisfied himself that it does *not* possess such properties, having employed it in two cases of recent blennorrhagia, internally and externally, without success, and being unsuccessful also in an attempt to treat primitive chancre with it. This announcement, we doubt not, will be quite pleasant to those agriculturists who had anticipated *a rise* in the price of guano, in consequence of the increased demand which was expected, when it was announced as an anti-venereal. L. H. S.

— We learn from Dr. C. F. HEYWOOD, the Secretary for New York of the New Sydenham Society, that the publications of that Society for 1859 are ready for subscribers.